$7.00 per volume
Single numbers $2.00

QUARTERLY

Volume V, Number 2
April, 1931

Founded in 1927 by Edward Bradford Titchener and Carl Murchison

THE JOURNAL OF GENERAL PSYCHOLOGY

Experimental, Theoretical, Clinical, and Historical Psychology

APRIL, 1931

Chaque article est suivi d'un résumé en français
Jedem Artikel wird ein Referat auf deutsch folgen

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Worcester, Massachusetts

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Entered as second-class matter December 1, 1927, at the post-office at Worcester, Mass., under the Act of March 3, 1879

THE CONFLICT BETWEEN ARISTOTELIAN AND GALILEIAN MODES OF THOUGHT IN CONTEMPORARY PSYCHOLOGY*†

From the Psychologisches Institut, Universität Berlin

KURT LEWIN

In the discussion of several urgent problems of current experimental and theoretical psychology I propose to review the development of the concepts of physics, and particularly the transition from the Aristotelian to the Galileian mode of thought. My purpose is not historical, rather do I believe that certain questions, of considerable importance in the reconstruction of concepts in present-day psychology, may be clarified and more precisely stated through such a comparison, which provides a view beyond the difficulties of the day.

I do *not* intend to infer by deduction from the history of physics what psychology "ought" to do. I am not of the opinion that there is only one empirical science, namely, physics; and the question whether psychology, as part of biology, is reducible to physics or is an independent science may here be left open.

Since we are starting from the point of view of the researcher, we shall, in our contrast of Aristotelian and Galileian concept formation, be less concerned with personal nuances of theory in Galileo and Aristotle than with certain rather ponderable differences in the modes of thought which determined the actual research of the mediaeval Aristotelians and of the post-Galileian physicists. Whether some particular investigator had previously shown the later sort of thinking in respect of some special point, or if some very modern speculations of the relativity theory should accord in some way with Aristotle's, is without relevance in the present connection.

In order to provide a setting especially for the theoretical treatment of the dynamic problems, I shall consider first the *general* characteristics of Aristotelian and Galileian physics and of modern psychology.

*Accepted for publication by Carl Murchison of the Editorial Board and received in the Editorial Office, May 20, 1930.

†English translation by D. K. Adams.

I. General Character of the Two Modes of Thought

A. In Physics

If one ask what is the most characteristic difference between "modern" post-Galileian physics and Aristotelian, one receives, as a rule, the following reply, which has had an important influence upon the scientific ideals of the psychologist: The concepts of Aristotelian physics were *anthropomorphic* and *inexact.* Modern physics, on the contrary, is quantitatively exact, and pure mathematical, functional relations now occupy the place of the former anthropomorphic explanations. These have given to physics that abstract appearance in which modern physicists are accustomed to take special pride.

This view of the development of physics is, to be sure, pertinent. But if one fixes one's attention less upon the "style" of the concepts employed, and more upon their actual functions as instruments for understanding the world, these differences appear to be more of a secondary nature, consequences of a deeper lying difference in the conception of the relationship between the world and the task of research.

1. *Aristotelian Concepts*

a. Their valuative character. As in all sciences, the detachment of physics from the universal matrix of philosophy and practice was only gradually achieved. Aristotelian physics is full of concepts which today we consider not only as specifically biological, but preeminently as *valuative* concepts. It abounds in specifically normative concepts taken from ethics, which occupy a place between valuative and non-valuative concepts: The "highest" forms of motions are circular and rectilinear, and they occur only in "heavenly" movements, those of the stars. The "earthly" sublunar world is endowed with motion of inferior types. There are similar valuative differences between causes: On one side there are the good or, so to speak, authorized forces of a body which come from its tendency toward perfection (τέλος), and on the other side the "disturbances" due to chance and to the opposing forces (βία) of other bodies.

This kind of classification in terms of values plays an extraordinarily important part in mediaeval physics. It classes many things with very slight or unimportant relationships together and separates things that objectively are closely and importantly related.

It seems obvious to me that this extremely "anthropomorphic"

mode of thought plays a large rôle in psychology, even to the present day. Like the distinction between earthly and heavenly, the no less valuative distinction between "normal" and "pathological" has for a long time sharply differentiated two fields of psychological fact, and thus separated phenomena which are fundamentally most nearly related.

No less important is the fact that value concepts completely dominate the conceptual setting of the special problems, or have until very recently done so. Thus, not till lately has psychology begun to investigate the structural (Gestalt) relations concerned in perception, thus replacing the concept of optical *illusion,* a concept derived not from psychological but from epistemological categories, which unwarrantedly lumps all these "illusions" together and sets them apart from the other phenomena of psychological optics. Psychology speaks of the "errors" of children, of "practice," of "forgetting," thus classifying whole groups of processes according to the value of their products, instead of according to the nature of the psychological processes involved. Psychology is, to be sure, beyond classifying events *only* on the basis of value when it speaks of "disturbances," of inferiority and superiority in development, or of the quality of performance on a test. On all sides there are tendencies to attack actual psychological processes. But there can hardly be any doubt that we stand now only at the beginning of this stage, that the same transitional concepts that we have seen in the Aristotelian physics to lie between the valuative and the non-valuative are characteristic of such antitheses as those of intelligence and feebleminded-ness, or of drive and will. The detachment of the conceptual structure of psychology from the utilitarian concepts of pedagogy, medicine, and ethics is only partly achieved.

It is quite possible, indeed I hold it to be probable, that the utility or performance concepts such as, for example, a "true" cognition versus an "error," may later acquire a legitimate sense. If that is the case, however, an "illusion" will have to be characterized not epistemologically but biologically.

b. Abstract classification. When the Galileian and post-Galileian physics disposed of the distinction between heavenly and earthly, and thereby extended the field of natural law enormously, it was not due solely to the exclusion of value concepts, but also to a changed interpretation of *classification.* For Aristotelian physics the membership of an object in a given class was of critical im-

portance, because for Aristotle the class defined the essence or essential nature of the object, and thus determined its behavior in both positive and negative respects.

This classification often took the form of paired opposites, such as cold and warm, dry and moist, and compared with present-day classification had a rigid, "absolute" character. In modern quantitative physics dichotomous classifications have been entirely replaced by *continuous gradations.* Substantial concepts have been replaced by functional concepts (1).

Here also it is not difficult to point out the analogous stage of development in contemporary psychology. The separation of intelligence, memory, and impulse bears throughout the characteristic stamp of Aristotelian classification; and in some fields, for example, in the analysis of feelings (pleasantness and unpleasantness), or of temperaments (13), or of drives (8), such dichotomous classifications as Aristotle's are even today of great significance. Only gradually do these classifications lose their importance and yield to a conception which seeks to derive the same laws for all these fields, and to classify the *whole* field on the basis of other, essentially *functional,* differences.

c. The concept of law. Aristotle's classes are *abstractly* defined as the sum total of those characteristics which a group of objects have in common. This circumstance is not merely a characteristic of Aristotle's logic, but largely determines his conception of *lawfulness* and *chance,* which seems to me so important to the problems of contemporary psychology as to require closer examination.

For Aristotle those things are lawful, conceptually intelligible, which occur *without exception.* Also, and this he emphasizes particularly, those are lawful which occur *frequently.* Excluded from the class of the conceptually intelligible as "mere chance" are those things which occur only *once,* individual events as such. Actually since the behavior of a thing is determined by its essential nature, and this essential nature is exactly the abstractly defined class (that is, the sum total of the common characteristics of a whole group of objects), it follows that each event, as a particular event, is chance, undetermined. For in these Aristotelian classes individual differences disappear.

The real source of this conception may lie in the fact that for Aristotelian physics *not all* physical processes possess the lawful character ascribed to them by post-Galileian physics. To the young

science of physics the universe it investigated appeared to contain as much that was chaotic as that which was lawful. The lawfulness, the intelligibility of physical processes was still narrowly limited. It was really present only in *some* processes, as, for example, the courses of the stars, but by no means in all the fleeting and transitory events of the earth. Just as for other young sciences, it was still a *question* for physics, whether and how far physical processes were subject to law. And this circumstance exercised its full effect on the formation of physical concepts, even though in philosophical "principle" the idea of general lawfulness already existed. In post-Galileian physics, with the elimination of the distinction between lawful and chance events, the necessity also disappeared of proving that the process under consideration was lawful. For Aristotelian physics, on the contrary, it was necessary to have criteria to decide *whether* or not a given event was of the lawful variety. Indeed the regularity with which similar events occurred in nature was used essentially as such a criterion. Only such events, as the celestial, which the course of history proves to be regular, or at least frequent, are subject to law; and only insofar as they are frequent, and hence "more" than individual events, are they conceptually intelligible. In other words, the ambition of science to understand the complex, chaotic, and unintelligible world, its faith in the ultimate decipherability of this world, was limited to such events as were *certified* by repetition in the course of history to possess a certain persistence and stability.

In this connection it must not be forgotten that Aristotle's emphasis on frequency (as a further basis for lawfulness, besides absolute regularity) represents, relative to his predecessors, a tendency toward the extension and concrete application of the principle of lawfulness. The "empiricist" Aristotle insists that not only the regular but the frequent is lawful. Of course, this only makes clearer his antithesis of individuality and law, for the individual event as such still lies outside the pale of the lawful and hence, in a certain sense, outside the task of science. Lawfulness remains limited to cases in which similar events recur, and classes (in Aristotle's abstract sense) reveal the essential nature of the events.

This attitude toward the problem of lawfulness in nature, which dominated mediaeval physics and from which even the opponents of Aristotelian physics, such as Bruno and Bacon, escaped only gradually, by small steps, had important consequences in several respects.

As will be clear from the preceding text, this concept of law-

fulness had throughout a quasi-statistical character. Lawfulness was considered as equivalent to the highest degree of generality, as that which occurs very often in the same way, as the extreme case of regularity, and hence as the perfect antithesis of the infrequent or of the particular event. The statistical determination of the concept of lawfulness is still clearly marked in Bacon, as when he tries to decide through his *"tabula praesentia"* whether a given association of properties is real (essential) or fortuitous. Thus he ascertains, for example, the numerical frequency of the cases in which the properties "warm" and "dry" are associated in everyday life. Less mathematically exact, indeed, but no less clear is this statistical way of thinking in the whole body of Aristotelian physics.

At the same time—and this is one of the most important consequences of the Aristotelian conception—regularity or particularity was understood entirely in *historical* terms.

The complete freedom from exceptions, the "always" which is found also in the later conceptions of physical lawfulness, still has here its original connections with the frequency with which similar cases have occurred in the *actual, historical* course of events in the everyday world. A crude example will make this clearer: light objects, under the conditions of everyday life, relatively frequently go up; heavy objects usually go down. The flame of the fire, at any rate under the conditions known to Aristotle, almost always goes upward. It is these frequency rules, within the limits of the climate, mode of life, etc., familiar to Aristotle, that determine the nature and tendency to be ascribed to each class of objects, and lead in the present instance to the conclusion that flames and light bodies have a tendency upward.

Aristotelian concept formation has yet another immediate relation to the geographically-historically given, in which it resembles, as do the valuative concepts mentioned above, the thinking of primitive man and of children.

When primitive man uses different words for "walking," depending upon its direction, north or south, or upon the sex of the walker, or upon whether the latter is going into or out of a house (5), he is employing a reference to the historical situation that is quite similar to the putatively "absolute" descriptions ("upward" or "downward") of Aristotle, the real significance of which is a sort of *geographic* characterization, a place definition relative to the earth's surface.[1]

[1]In the following pages we shall frequently have to use the term "historic-geographic." This is not in common usage, but it seems to me inaccurate

The original connection of the concepts with the "actuality," in the special sense of the given historic-geographic circumstances, is perhaps the most important feature of Aristotelian physics. It is from this almost more even than from its teleology that his physics gets its general anthropomorphic character. Even in the minute particulars of theorizing and in the actual conduct of research it is always evident, not only that physical and normative concepts are still undifferentiated, but that the formulation of problems and the concepts that we would today distinguish, on the one hand, as "historic"[2] and, on the other, as non-historic or "systematic" are inextricably interwoven. (Incidentally, an analogous confusion exists in the early stages of other sciences, for example in economics.)

From these conceptions also the attitude of Aristotelian physics toward lawfulness takes a new direction. So long as lawfulness remained limited to such processes as occurred repeatedly in the same way, it is evident, not only that the young physics still lacked the courage to extend the principle to all physical phenomena, but also that the concept of lawfulness still had a fundamentally historic, a temporally particular, significance. Stress was laid not upon the "general validity" which modern physics understands by lawfulness, but upon the events in the historically given world which displayed the required stability. The highest degree of lawfulness, beyond mere frequency (ἐπὶ τὸ πολὺ), was characterized by the idea of always eternal (ἀεὶ). That is, the stretch of historic time for which constancy was assumed was extended to eternity. General validity of *law* was not yet clearly distinguished from eternity of *process*. Only permanence, or at least frequent repetition, was proof of more than momentary validity. Even here in the idea of eternity, which seems to transcend the historical, the connection with immediate historic actuality is still obvious, and this close connection was characteristic of the "empiricist" Aristotle's method and concepts.

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to contrast historic and systematic questions. The real opposition is between "type" (of object, process, situation) and "occurrence." And for concepts that deal with "occurrence," the reference to "absolute" geographic space-coordinates is just as characteristic as that to "absolute" time-coordinates by means of dates.

At the same time, the concept of the "geographic" should be understood in such a general sense as to refer to juxtaposition, correlative to historical *succession*, that the concept is applicable, for example, to psychical events.

[2]There is no term at present in general use to designate non-historic problem formulations. I here employ the term "systematic," meaning thereby, not "ordered," but collectively non-historic problems and laws such as those which form the bulk of present-day physics. (*Cf. infra.*)

Not only in physics but in other sciences, for example, in economics and biology, it can be clearly seen how in certain early stages the tendency to empiricism, to the collection and ordering of "facts," carries with it at the same time a tendency to historical concept formation, to excessive valuation of the historical.

2. *Galileian Physics.* From the point of view of this sort of empiricism the concept formation of Galileian and post-Galileian physics must seem curious and even paradoxical.

As remarked above, the use of mathematical tools and the tendency to exactness, important as they are, cannot be considered the real substance of the difference between Aristotelian and Galileian physics. It is indeed quite possible to recast in mathematical form the essential content of, for example, the dynamic ideas of Aristotelian physics. (Cf. *infra.*) It is conceivable that the development of physics could have taken the form of a mathematical rendition of Aristotelian concepts such as is actually taking place in psychology today. In reality, however, there were only traces of such a tendency, such as Bacon's quasi-statistical methods mentioned above. The main development took another direction and proved to be a change of content rather than a mere change of form.

The same considerations apply to the "exactness" of the new physics. It must not be forgotten that in Galileo's time there were no clocks of the sort we have today, that these first became possible through the knowledge of dynamics founded upon Galileo's work (9). Even the methods of measurement used by Faraday in the early investigations of electricity show how little exactness, in the current sense of *precision* to such and such a decimal place, had to do with these critical stages in the development of physics.

The real sources of the tendency to quantification lie somewhat deeper, namely in a new conception by the physicist of the nature of the physical world, in an extension of the demands of physics upon itself in the task of understanding the world, and in an increased faith in the possibility of their fulfillment. These are radical and far-reaching changes in the fundamental ideas of physics, and the tendency to quantification is simply one of their expressions.

a. Homogenization. The outlook of a Bruno, a Kepler, or a Galileo is determined by the idea of a comprehensive, all-embracing unity of the physical world. The same law governs the courses of the stars, the falling of stones, and the flight of birds. This "homogenization" of the physical world with respect to the validity of law

deprives the division of physical objects into rigid abstractly defined classes of the critical significance it had for Aristotelian physics, in which membership in a certain conceptual class was considered to determine the physical nature of an object.

Closely related to this is the loss in importance of logical dichotomies and conceptual antitheses. Their places are taken by more and more fluid transitions, by gradations which deprived the dichotomies of their antithetical character and represent in logical form a transition stage between the class concept and the serial concept (1).

b. Genetic concepts. This dissolution of the sharp antitheses of rigid classes was greatly accelerated by the coeval transition to an essentially functional way of thinking, to the use of *conditional-genetic* concepts. For Aristotle the immediate perceptible appearance, that which present-day biology terms the *phenotype,* was hardly distinguished from the properties that determine the object's dynamic relations. The fact, for example, that light objects relatively frequently go upward sufficed for him to ascribe to them an upward tendency. With the differentiation of phenotype from *genotype* or, more generally, of "descriptive" from "conditional-genetic" (7) concepts, and the shifting of emphasis to the latter, many old class distinctions lost their significance. The orbits of the planets, the free falling of a stone, the movement of a body on an inclined plane, the oscillation of a pendulum, which if classified according to their phenotypes would fall into quite different, indeed into antithetical classes, prove to be simply various expressions of the same law.

c. Concreteness. The increased emphasis upon the quantitative which seems to lend to modern physics a formal and abstract character is not derived from any tendency to logical formality. At the same time as the development of the problem of classification, or rather, much earlier, the tendency to a full description of the concrete actuality, even that of the particular case, was influential, a circumstance which should be especially emphasized in connection with present-day psychology. The particular object in all departments of science is determined not only in kind and thereby qualitatively, but it possesses each of its properties in a special intensity or to a definite degree. So long as one rewards as important and conceptually intelligible only such properties of an object as are common to a whole group of objects, the individual differences of degree remain without scientific relevance, for in the abstractly defined classes these differences more or less disappear. With the

mounting aspirations of research toward an understanding of actual events and particular cases, the task of describing the differences of degree that characterized individual cases had necessarily to increase in importance, and finally required actual quantitative determination.

It is the increased desire, and also the increased ability, to comprehend *concrete particular cases,* and to comprehend them fully, which, together with the idea of the *homogeneity* of the physical world and that of the *continuity* of the properties of its objects, constituted the main impulse to the increasing quantification of physics.

d. Paradoxes of the new empiricism. This tendency toward the closest possible contact with actuality, which today is usually regarded as characteristic and ascribed to an "anti-speculative" tendency, led to a mode of concept formation diametrically opposed to that of Aristotle, and, surprisingly enough, involved also the direct antithesis of his "empiricism."

The Aristotelian concepts show, as we have seen above, an immediate reference to the historically given reality and to the actual course of events. This reference, or at any rate this *immediate* reference to the historically given, is lacking in modern physics. The fact, so decisively important for Aristotelian concepts, that a certain process was only once, or very frequently, or invariably repeated in the course of history, is practically irrelevant to the most essential questions of modern physics.[3] This circumstance is considered fortuitous or "merely historical."

The law of falling bodies, for example, does *not* assert that bodies very frequently fall downward. It does *not* assert that the event to which the formula, $s = 1/2\ gt^2$, applies, the "free and unimpeded fall" of a body, occurs regularly or even frequently in the actual history of the world. Whether the event described by the law occurs rarely or often has nothing to do with the law. Indeed, in a certain sense, the law refers only to cases that are *never* realized, or only approximately realized, in the actual course of events. Only in experiment, that is, under artificially constructed conditions, do cases occur which approximate the event with which the law is concerned. The propositions of modern physics, which are often considered to be "anti-speculative" and "empirical," unquestionably have in comparison with Aristotelian empiricism a much less empirical, a much more

[3]So far as it is not immediately concerned with an actual "History of the Heavens and the Earth" or a geography. (*Cf. infra.*)

constructive character than the Aristotelian concepts based immediately upon historical actuality.

B. Psychology

Here we are confronted by questions which, as real problems of actual research and of theory, have strongly influenced the development of psychology and which constitute the most fundamental grounds of its present crisis.

The concepts of psychology, at least in certain decisive respects, are thoroughly Aristotelian in their actual content, even though in many respects their form of presentation has been somewhat "civilized," so to speak. The present struggles and theoretical difficulties of psychology resemble in many ways, even in their particulars, the difficulties which culminated in the conquest over Aristotelian ways of thinking in physics.

1. *Aristotelian Concepts*

a. Fortuitousness of the individual case. The concept formation of psychology is dominated, just as was that of Aristotelian physics, by the question of regularity and indeed of regularity in the sense of frequency. This is obvious in its immediate attitude toward particular phenomena as well as in its attitude toward lawfulness. If, for example, one show a film of a concrete incident in the behavior of a certain child, the first question of the psychologist usually is: "Do all children do that, or is it at least common?" And if one must answer this question in the negative the behavior involved loses for that psychologist all or almost all claim to scientific interest. To pay attention to such an "exceptional case" seems to him a scientifically unimportant bit of folly.

The real attitude of the investigator toward particular events and the problem of individuality is perhaps more clearly expressed in this actual behavior than in many theories. The individual event seems to him fortuitous, unimportant, scientifically indifferent. It may, however, be some extraordinary event, some tremendous experience, something that has critically determined the destiny of the person involved, or the appearance of an historically significant personality. In such a case it is customary to emphasize the "mystical" character of all individuality and "originality," comprehensible only to "intuition," or at least not to science.

Both of these attitudes toward the particular event lead to the

same conclusion: that that which does not occur repeatedly lies outside the realm of the comprehensible.

b. Lawfulness as frequency. The esteem in which frequency is held in present-day psychology is due to the fact that it is still considered a *question* whether and how far the psychical world is lawful, just as in Aristotelian physics it was due to a similar uncertainty about lawfulness in the physical world. It is not necessary here to describe at length the vicissitudes of the thesis of the lawfulness of the psychic in philosophical discussion. It is sufficient to recall that even at present there are many tendencies to limit the operation of law to certain "lower" spheres of psychical events. For us it is more important to note that the field which is considered lawful, not in principle, but in the actual research of psychology—even of experimental psychology—has only been extended very gradually. If psychology has only very gradually and hesitantly pushed beyond the bounds of sensory psychology into the fields of will and affect, it is certainly not due only to technical difficulties, but mainly to the fact that in this field actual repetition, a recurrence of the same event, is not to be expected. And this repetition remains, as it did for Aristotle, to a large extent the basis for the assumption of the lawfulness or intelligibility of an event.

As a matter of fact, any psychology that does not recognize lawfulness as inherent in the nature of the psychic, and hence in *all* psychical processes, even those occurring only once, *must* have criteria to decide, like Aristotelian physics, whether or not it has in any given case to deal with lawful phenomena. And, again, just as in Aristotelian physics, frequency of recurrence is taken as such a criterion. It is evidence of the depth and momentum of this connection (between repetition and lawfulness) that it is even used to define experiment, a scientific instrument which, if not directly opposed to the concepts of Aristotelian physics, is at least significant only in relatively modern times.[4] Even for Wundt repetition inhered in the concept of experiment. Only in recent years is psychology beginning to give up this requirement, which withholds a large field of the psychic from experimental investigation.

But even more important perhaps than the restriction of experimental investigation is the fact that this extravagant valuation of repetition (i.e., considering frequency as the criterion and expression

[4]The Greeks, of course, *knew* of experiment.

of lawfulness) dominates the formation of the concepts of psychology, particularly in its younger branches.

Just as in Aristotelian physics, contemporary child psychology regards as characteristic of a given age, and the psychology of emotion as characteristic of a given expression, that which a group of individual cases have in common. This abstract Aristotelian conception of the class determines the kind and dominates the procedure of classification.

c. Class and essence. Present-day child psychology and affect psychology also exemplify clearly the Aristotelian habit of considering the abstractly defined classes as the essential nature of the particular object and hence as an "explanation" of its behavior. Whatever is common to children of a given age is set up as the fundamental character of that age. The fact that three-year-old children are quite often negative is considered evidence that negativism is inherent in the nature of three-year-olds, and the concept of a negativistic age or stage is then regarded as an explanation (though perhaps not a complete one) for the appearance of negativism in a given particular case!

Quite analogously, the concept of drives, for example, the hunger drive or the maternal instinct, is nothing more than the abstract selection of the features common to a group of acts that are of relatively frequent occurrence. This abstraction is set up as the essential reality of the behavior and is then in turn used to explain the frequent occurrence of the instinctive behavior, for example, of the care of infant progeny. Most of the explanations of expression, of character, and of temperament are in a similar state. Here, as in a great many other fundamental concepts, such as that of ability, talent, and similar concepts employed by the intelligence testers, present-day psychology is really reduced to explanation in terms of Aristotelian "essences," a sort of explanation which has long been attacked as faculty psychology and as circular explanation, but for which no other way of thinking has been substituted.

d. Statistics. The classificatory character of its concepts and the emphasis on frequency are indicated methodologically by the commanding significance of statistics in contemporary psychology. The statistical procedure, at least in its commonest application in psychology, is the most striking expression of this Aristotelian mode of thinking. In order to exhibit the common features of a given group of facts, the *average* is calculated. This average acquires a

representative value, and is used to characterize (as "mental age") the properties of "the" two-year-old child. Outwardly, there is a difference between contemporary psychology, which works so much with numbers and curves, and the Aristotelian physics. But this difference, characteristically enough, is much more a difference in the technique of execution than in the actual content of the concepts involved. Essentially, the statistical way of thinking, which is a necessary consequence of Aristotelian concepts, is also evident in Aristotelian physics, as we have already seen. The difference is that, owing to the extraordinary development of mathematics and of general scientific method, the statistical procedure of psychology is clearer and more articulate.

All the efforts of psychology in recent years toward exactness and precision have been in the direction of refinement and extension of its statistical methods. These efforts are quite justified insofar as they indicate a determination to achieve an adequate comprehension of the full reality of mental life. But they are really founded, at least in part, on the ambition to demonstrate the scientific status of psychology by using as much mathematics as possible and by pushing all calculations to the last possible decimal place.

This formal extension of the method has not changed the underlying concepts in the slightest: they are still thoroughly Aristotelian. Indeed, the mathematical formulation of the method only consolidates and extends the domination of the underlying concepts. It unquestionably makes it more difficult to see their real character and hence to supplant them with others; and this is a difficulty with which Galileian physics did not have to contend, inasmuch as the Aristotelian mode of thought was not then so entrenched and obscured in mathematics. (*Cf. supra.*)

e. Limits of knowledge. Exceptions. Lawfulness is believed to be related to regularity, and considered the antithesis of the individual case. (In terms of the current formula, lawfulness is conceived as a correlation approaching $r = \pm 1$.) So far as the psychologist agrees at all to the validity of psychological propositions, he regards them as only *regularly* valid, and his acceptance of them takes such a form that one remains aware of a certain distinction between mere regularity and full lawfulness; and he ascribes to biological, and above all to psychological propositions (in contrast to physical), only regularity. Or else lawfulness is believed to be only

the *extreme* case of regularity,[5] in which case all differences (between lawfulness and regularity) disappear in principle while the necessity of determining the degree of regularity still remains.

The fact that lawfulness and individuality are considered antitheses has two sorts of effect on actual research. It signifies in the first place a limitation of research. It makes it appear hopeless to try to understand the real, unique, course of an emotion or the actual structure of a particular individual's personality. It thus reduces one to a treatment of these problems in terms of mere averages, as exemplified by tests and questionnaires. Any one to whom these methods appear inadequate usually encounters a weary scepticism or else a maudlin appreciation of individuality and the doctrine that this field, from which the recurrence of similar cases in sufficient numbers is excluded, is inaccessible to scientific comprehension and requires instead sympathetic intuition. In both cases the field is withdrawn from experimental investigation, for qualitative properties are considered as the direct opposite of lawfulness. The manner in which this view is continually and repeatedly advanced in the discussion of experimental psychology resembles, even to its particulars, the arguments against which Galileian physics had to struggle. How, it was urged at that time, can one try to embrace in a single law of motion such qualitatively different phenomena as the movements of the stars, the flying of leaves in the wind, the flight of birds, and the rolling of a stone downhill. But the opposition of law and individual corresponded so well with the Aristotelian conception and with the primitive mode of thinking which constituted the philosophy of everyday life, that it appears often enough in the writings of the physicists themselves, *not,* however, in their physics but in their philosophy.[6]

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[5]As is well known, the concept of possible exceptions and the merely statistical validity of laws has very recently been revived in physical discussion. Even if this view should finally be adopted, it would not in any way mean a return to Aristotelian concepts. It suffices here to point out that even in that event, it would not involve setting apart within the physical world a class of events on the basis of its "degree" of lawfulness, but the whole physical universe would be subject only to a statistical lawfulness. On the relation of this statistical view to the problem of precision of measurement, see Lewin (7).

[6]To avoid misunderstanding, the following should be emphasized: when we criticize the opposition of individual and law, as is customary in psychology, it does not mean that we are unaware of the complex problems of the concept of individuality.

The conviction that it is impossible wholly to comprehend the individual case as such implies, in addition to this limitation, a certain laxity of research: it is satisfied with setting forth mere regularities. The demands of psychology upon the *stringency* of its propositions go no farther than to require a validity "in general," or "on the average," or "as a rule." The "complexity" and "transitory nature" of life processes make it unreasonable, it is said, to require complete, exceptionless, validity. According to the old saw that "the exception proves the rule," *psychology does not regard exceptions as counter-arguments so long as their frequency is not too great.*

The attitude of psychology toward the concept of lawfulness also shows clearly and strikingly the Aristotelian character of its mode of thought. It is founded on a very meager confidence in the lawfulness of psychological events, and has for the investigator the added charm of not requiring too high a standard of validity in his propositions or in his proofs of them.

f. Historic-geographic concepts. For the view of the nature of lawfulness and for the emphasis upon repetition which we have seen to be characteristic of Aristotelian physics, in addition to the motives which we have just mentioned, the *immediate reference* to the concerned "actuality" in the historic-geographic sense was fundamental. Likewise, and this is evidence of the intimacy with which these modes of thought are related, present-day psychology is largely dominated by the same immediate reference to the historic-geographic datum. The historical bent of psychological concepts is again not always immediately obvious as such, but is bound up with non-historic, systematic concepts and undifferentiated from them. This quasi-historical set forms, in my opinion, the central point for the understanding and criticism of this mode of concept formation.

Although we have criticized the "statistical" mode of thought, the particular formulae used are not ultimately important to the questions under discussion. It is not the fact that an arithmetic mean is taken, that one adds and divides, that is the object of the present critique. These operations will certainly continue to be used extensively in the future of psychology. The critical point is, not that statistical methods are applied, but *how* they are applied, and especially, what cases are combined into groups.

In contemporary psychology the reference to the historic-geographic datum and the dependence of the conclusions upon frequency of actual occurrence are striking. Indeed, so far as immediate reference

to the historic datum is concerned, the way in which the nature of the one-, two-, or three-year-old child is arrived at through the calculation of statistical averages corresponds exactly to Bacon's collection of the given cases of dryness in his *tabulae praesentiae*. To be sure, there is a certain very crude concession made in such averages, to the requirements of non-historic concepts: patently pathological cases, and sometimes even cases in which an "unusual" environment is concerned, are usually excluded. Apart from this consideration, the exclusion of the most extreme abnormalities, the determination of the cases to be placed in a statistical group is essentially on historic-geographic grounds. For a group defined in historic-geographic terms, perhaps the one-year-old children of Vienna or New York in the year 1928, averages are calculated which are doubtless of the greatest significance to the historian or to the practical school man, but which do not lose their dependence upon the "accidents" of the historic-geographic given even though one go on to an average of the children of Germany, of Europe, or of the whole world, or of a decade instead of a year. *Such an extension of the geographic and historic basis does not do away with the specific dependence of this concept upon the frequency with which the individual cases occur within historically-geographically defined fields.*

Mention should have been made earlier of that refinement of statistics which is founded upon a *restriction* of the historic-geographic basis, as, for example, a consideration of the one-year-old children of a proletarian quarter of Berlin in the first years after the War. For such groupings usually are based on the qualitative individuality of the concrete cases as well as upon historic-geographic definitions. But even such limitations really contradict the spirit of statistics founded on frequency. Even they signify methodologically a certain shift to the concrete particulars. Incidentally, one must not forget that even in the extreme case of such refinement, perhaps in the statistical investigation of the "only child," the actual definition is in terms of historic-geographic or at best of sociological categories; that is, according to criteria which combine into a single group cases that psychologically are very different or even antithetical. Such statistical investigations are consequently unable as a rule to give an explanation of the dynamics of the processes involved.

The immediate reference to the historically given actuality which is characteristic of Aristotelian concept formation is evident also in the discussion of experiment and nearness to life conditions. Certainly

one may justly criticize the simple reaction experiments, the beginnings of the experimental psychology of the will or the experiments of reflexology on the ground of their wide divergence from the conditions of life. But this divergence is based in large part upon the tendency to investigate such processes as do not present the individual peculiarities of the particular case but which, as "simple elements" (perhaps the simplest movements), are common to all behavior, or which occur, so to speak, in everything. In contrast, approximation of life conditions is required, say of the psychology of will. By this is usually meant that it should investigate those cases, impossible to produce experimentally, in which the most important decisions of life are made. And here also we are confronted by an orientation toward the historically significant. It is a requirement which, if transferred to physics, would mean that it would be incorrect to study hydrodynamics in the laboratory; one must rather investigate the largest rivers in the world. Two points then stand out in the field of theory and law: the high valuation of the historically important and disdain of the "ordinary;" in the field of experiment, the choice of processes which occur frequently (or are common to many events). Both are indicative in like measure of that Aristotelian mixing of historical and systematic questions which carries with it for the systematic the connection with the abstract classes and the neglect of the full reality of the concrete case.

2. *Galileian Concept Formation.* Opposed to Aristotelian concept formation which I have sought briefly to characterize, there is now evident in psychology a development which appears occasionally in radical or apparently radical tendencies, more usually in little half-steps, sometimes falling into error (especially when it tries most exactly to follow the example of physics), but which on the whole seems clearly and irresistibly to be pushing on to modifications which may ultimately mean nothing less than a transition from Aristotelian to Galileian concept formation.

a. No value concepts. No dichotomies. Unification of fields. The most important general circumstances which paved the way for Galileian concepts in physics are clearly and distinctly to be seen in present-day psychology.

The conquest over *"valuative,"* "anthropomorphic" classifications of phenomena on bases other than the nature of the mental process itself (cf. p. 142) is not by any means complete, but in many fields, especially in sensory psychology, at least the chief difficulties are past.

As in physics, the grouping of events and objects into paired opposites and similar logical dichotomies is being replaced by groupings with the aid of *serial* concepts which permit of continuous variation, partly owing simply to wider experience and the recognition that transition stages are always present.

This has gone farthest in sensory psychology, especially in psychological optics and acoustics, and lately also in the domain of smell. But the tendency toward this change is also evident in other fields, for example, in that of feeling.

Freud's doctrine especially—and this is one of its greatest services —has contributed largely to the abolition of the boundary between the normal and the pathological, the ordinary and the unusual, and hereby furthered the *"homogenization"* (cf. p. 148) of all the fields of psychology. This process is certainly still far from complete, but it is entirely comparable to that introduced in modern physics by which "heavenly" and "earthly" processes were united.

Also in child and animal psychology the necessity is gradually disappearing of choosing between the two alternatives of regarding the child as a little adult, the animal as an undeveloped inferior human, or else trying to establish an unbridgeable gap between the child and adult, animal and man. This homogenization is becoming continually clearer in all fields, and it is not a purely "philosophical" insistence upon some sort of abstract fundamental unity but influences concrete research in which differences are fully preserved.

b. Unconditional general validity of psychological laws. The clearest and most important expression of increasing homogeneity, beside the transition from class to serial concepts, is the fact that the validity of particular psychological laws is no longer limited to particular fields, as it was once limited to the "normal human adult" on the ground that anything might be expected of psychopathics or of geniuses, or that in such cases "the same laws do not hold." It is coming to be realized that every psychological law must hold without exception.

In actual content, this transition to the concept of strict exceptionless lawfulness signifies at once the same final and all-embracing homogenization and harmonization of the whole field that gave to Galileian physics its intoxicating feeling of infinite breadth, because it does not, like the abstract class concepts, level out the rich variety of the world and because a single law embraces the whole field.

Tendencies toward a homogeneity based upon the exceptionless

validity of its laws have become evident in psychology only very recently, but they open up an extraordinarily wide perspective.[7]

The investigation of the laws of structure—particularly the experimental investigation of wholes—has shown that the same laws hold not only within different fields of psychological optics but also in audition, and in sensory psychology in general. This in itself constitutes a large step in the progress toward homogeneity.

Further, the laws of optical figures and of intellectual insight have turned out to be closely related. Important and similar laws have been discovered in the experimental investigation of behavioral wholes, of will processes, and of psychological needs. In the fields of memory and expression, psychological development appears to be analogous. In short, the thesis of the general validity of psychological laws has very recently become so much more concrete, particular laws have shown such capacity for fruitful application to fields that at first were qualitatively completely separated, that the thesis of the homogeneity of psychic life in respect to its laws gains tremendously in vigor and is destroying the boundaries of the old separated fields.[8]

c. Mounting ambitions. Methodologically also the thesis of the exceptionless validity of psychological laws has a far-reaching significance. It leads to an extraordinary increase in the demands made upon *proof*. It is no longer possible to take exceptions lightly. They do not in any way "prove the rule," but on the contrary are completely valid disproofs, even though they are rare, indeed, so

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[7]The association psychology contains an attempt at this sort of homogeneity, and it has really been of essential service in this direction. Similarly, in our time reflexology and behaviorism have contributed to the homogenization of "man and animal" and of "bodily and mental." But the Aristotelian view of lawfulness as regularity (without which it would have been impossible to support the law of association) brought this attempt to nothing. Consequently, the experimental association psychology, in its attempt at the end of the nineteenth century to derive the whole mental life from a single law, displayed the circular and at the same time abstract character that is typical of the speculative early stages of a science, and of Aristotelian class concepts.

Indeed, it seems almost as if, because of the great importance of frequency and repetition for Aristotelian methodological concepts, the law of association had been designed to make use of these as the actual *content* of psychological principles, inasmuch as frequent repetition is regarded as the most important cause of mental phenomena.

[8]For this section compare especially Wertheimer (14), Köhler (4), Koffka (2), and Lewin (6). A review of the special researches is found in Köhler (3).

long as one single exception is demonstrable. The thesis of general validity permits of no exceptions in the entire realm of the psychic, whether of child or adult, whether in normal or pathological psychology.

On the other hand, the thesis of exceptionless validity in psychological laws makes available to investigation, especially to experiment, such processes as do *not* frequently recur in the same form, as, for example, certain affective processes.

d. From the average to the "pure" case. A clear appreciation of this circumstance is still by no means habitual in psychology. Indeed, from the earlier, Aristotelian point of view the new procedure may even seem to conceal the fundamental contradiction we have mentioned above. One declares that one wants to comprehend the full concrete reality in a higher degree than is possible with Aristotelian concepts, and yet considers this reality in its actual historical course and its given geographical setting as really "accidental." The general validity, for example, of the law of movement on an inclined plane is not established by taking the average of as many cases as possible of real stones actually rolling down hills, and then considering this average as the most probable case.[9] It is based rather upon the "frictionless" rolling of an "ideal" sphere down an "absolutely straight" and hard plane, that is, upon a process that even the laboratory can only approximate, and which is most extremely improbable in daily life. One declares that one is striving for general validity and concreteness, yet uses a method which, from the point of view of the preceding epoch, disregards the historically given facts and depends entirely upon individual accidents, indeed upon the most pronounced "exceptions."

How physics arrives at this procedure, which strikes the Aristotelian views of contemporary psychology as doubly paradoxical, begins to become intelligible when one envisages the necessary methodological consequences of the change in the ideas of the extent of lawfulness. When lawfulness is no longer limited to cases which occur regularly or frequently but is characteristic of *every* physical event, the necessity disappears of demonstrating the lawfulness of

[9]In psychology it is asserted, often with special emphasis, that one obtains, perhaps from the construction of baby tests, a representation of the "general human," through the fact that those processes are selected which occur most frequently in the child's *daily life*. Then one may expect with sufficient probability that the child will spontaneously display similar behavior in the test.

an event by some special criterion, such as its frequency of occurrence. Even a "particular case" is then *assumed,* without more ado, to be lawful. Historical rarity is no disproof, historical regularity no proof of lawfulness. For the concept of lawfulness has been quite detached from that of regularity; the concept of the complete absence of exceptions to laws is strictly separated from that of historical constancy (the "forever" of Aristotle).[10]

Further, the content of a law cannot then be determined by the calculation of averages of historically given cases. For Aristotle the nature of a thing was expressed by the characteristics common to the historically given cases. Galileian concepts, on the contrary, which regard historical frequency as "accident," must also consider it a matter of chance which properties one arrives at by taking averages of historical cases. If the concrete event is to be comprehended and the thesis of lawfulness without exception is to be not merely a philosophical maxim but determinative of the mode of actual research, there must be another possibility of penetrating the nature of an event, some other way than that of ignoring all individual peculiarities of concrete cases. The solution of this problem may only be obtained by the elucidation of the paradoxical procedures of Galileian method through a consideration of the problems of dynamics.

II. Dynamics

A. *Changes in the Fundamental Dynamic Concepts of Physics*

The dynamic problems of physics were really foreign to the Aristotelian mode of thought. The fact that dynamic problems had throughout such great significance for Galileian physics permits us to regard it as a characteristic consequence of the Galileian mode of thought (10). As always, it involved not merely a superficial

[10]The contrast between Aristotelian and Galileian views of lawfulness and the difference in their methods may be briefly tabulated as follows:

	For Aristotle	For Galileo
1. The regular is	lawful	lawful
The frequent is	lawful	lawful
The individual case is	chance	lawful
2. Criteria of lawfulness are	regularity frequency	not required
3. That which is common to the historically occurring cases is	an expression of the nature of the thing	an accident, only "historically" conditioned

shift of interest, but a change in the content of the theories. Even Aristotle emphasized "becoming," as compared with his predecessors. It is perhaps more correct to say that in the Aristotelian concepts statics and dynamics are not yet differentiated. This is due especially to certain fundamental assumptions.

1. *Teleology and Physical Vectors.* A leading characteristic of Aristotelian dynamics is the fact that it explained events by means of concepts which we today perceive to be specifically biological or psychological: *every object tends, so far as not prevented by other objects, toward perfection,* toward the realization of its own nature. This nature is for Aristotle, as we have already seen, that which is common to the "class" of the object. So it comes about that the class for him is at the same time the concept and the goal (Τέλος) of an object.

This teleological theory of physical events does not show only that biology and physics are not yet separated. It indicates also that the dynamics of Aristotelian physics resembles in essential points the animistic and artificial mode of thought of primitive man, which views all movement as life and makes artificial "manufacture" the prototype of existence. For, in the case of manufactured things, the maker's idea of the object is, in one sense, both the cause and the goal of the event.

Further, for Aristotelian concepts the *cause* of a physical event was very closely related to psychological "drives": the object strives toward a certain goal; so far as movement is concerned, it tends toward the place appropriate to its nature. Thus heavy objects strive downward, and, indeed, the heavier the more strongly, while light objects strive upward.

It is customary to dismiss these Aristotelian physical concepts by calling them "anthropomorphic." But perhaps it would be better, when we consider that the same fundamental dynamic ideas are today completely dominant in psychology and biology, to examine the actual content of the Aristotelian theses as far as possible independently of the "style" of their presentation.

It is customary to say that teleology assumes a *direction of events toward a goal,* which causal explanation does not recognize, and to see in this the most essential difference between "teleological" and "causal" explanation. But this sort of view is inadequate, for the causal explanation of modern physics uses directed quantities, mathematically described vectors. Physical "force," which is defined

as "the cause of a physical change," is considered a directed, vectorial factor. In the employment of vectorial factors as the foundation of dynamics there is thus no difference between the modern and the Aristotelian view.

The real *difference* lies rather in the fact that *the kind and direction of the physical vectors in Aristotelian dynamics are completely determined in advance by the nature of the object concerned.* In modern physics, on the contrary, the existence of a physical vector always depends upon the *mutual relations of several physical facts,* especially upon the relation of the object to its environment.[11]

2. *Significance of the Whole Situation in Aristotelian and Galileian Dynamics.* For Aristotelian concepts, the *environment* plays a part only insofar as it may give rise to *"disturbances,"* "forced" modifications of the processes which follow from the nature of the object concerned. The vectors which determine an object's movements are completely determined by the object. That is, they do not depend upon the relation of the object to the environment, and they belong to that object once for all, *irrespective of its surroundings at any given time.* The tendency of light bodies to go up resided in the bodies themselves; the downward tendency of heavy objects was seated in those objects. In modern physics, on the contrary, not only is the "upward tendency" of a lighter body derived from the relation of this body to its environment, but the "weight" itself of the body depends upon such a relation.

This decisive revolution comes to clear expression in Galileo's classic investigations of the law of falling bodies. The mere fact that he did not investigate the heavy body itself, but the process of "free falling or movement on an inclined plane" signifies a transition to concepts which can be defined only by reference to a certain sort of *situation* (namely, the presence of a plane with a certain inclination or of an unimpeded vertical extent of space through which to fall). The idea of investigating free falling, which is too rapid for satisfactory observation, by resorting to the slower movement upon an inclined plane, presupposes that the dynamics of the event is no longer related to the isolated object as such, but is seen to be dependent upon the whole situation in which the event occurs.

Galileo's procedure, in fact, includes a penetrating investigation of precisely the situation factors. The slope of the inclined plane,

[11]Naturally this applies also to "internal causes" which involve the mutual relation of the parts of a physical system.

i.e., the proportion of height to length, is defined. The list of situations involved (free falling, movement on an inclined plane, and horizontal movement) is exhausted and, by varying the inclination, classified. The dependence of the essential features of the event (for example, its velocity) upon the essential properties of the situation (the slope of the plane) becomes the conceptual and methodological center of importance.

This view of dynamics does not mean that the nature of the object becomes insignificant. The properties and structure of the object involved remain important also for the Galileian theory of dynamics. But the situation assumes as much importance as the object. Only by the concrete *whole which comprises the object and the situation are the vectors which determine the dynamics of the event defined.*

In carrying out this view, Galileian physics tried to characterize the individuality of the concerned total situation as concretely and accurately as possible. This is an exact reversal of Aristotelian principles. The dependence of an event upon the situation in which it occurs means for the Aristotelian mode of thought, which wants to ascertain the "general" by seeking out the like features of many cases, nothing more than a disturbing force. The changing situations appear as something fortuitous that disturbs and obscures the essential nature. It was therefore valid and customary to *exclude the "influence of the situation" as far as possible,* to "abstract" from the situation, in order to understand the essential nature of the object and the direction of its goal.

3. *Getting Rid of the Historical Bent.* The actual investigation of that sort of vectors obviously presupposes that the processes involved occur with a certain *regularity* or frequency. (Cf. p. 145, *supra.*) For otherwise an exclusion of the differences of the situation would leave no similarities. If one start from the fundamental concepts of Aristotelian dynamics, the investigation of the dynamics of a process must be more difficult—one might think here of emotion in psychology—the more it depends upon the nature of the situation concerned. The single event becomes thereby unlawful in principle because there is no way of investigating its dynamics.

The *Galileian* method of determining the dynamics of a process is directly opposed to this procedure. Since the dynamics of the process depend not only upon the object but also and primarily upon the situation, it would be nonsensical to try to obtain general laws of

processes by excluding the influence of the situations as far as possible. It becomes silly to bring in as many as possible different situations and regard only those factors as generally valid that are observed "under all circumstances," in any and every situation. It must, on the contrary, become important to comprehend *the whole situation involved, with all its characteristics, as precisely as possible.*

The step from particular case to law, from "this" event to "such" an event, no longer requires the confirmation by historical regularity that is characteristic of the Aristotelian mode of thought. *This* step to the general is automatically and immediately given by the principle of the exceptionless lawfulness of physical events.[12] What is now important to the investigation of dynamics is not to abstract from the situation, but to hunt out those situations in which the determinative factors of the total dynamic structure are most clearly, distinctly, and purely to be discerned. *Instead of a reference to the abstract average of as many historically given cases as possible, there is a reference to the full concreteness of the particular situations.*

We cannot here examine in great detail the question, why not all situations are equally useful for the investigation of dynamics, why certain situations possess a methodological advantage and why as far as possible these are experimentally set up. Only one circumstance requires elucidation, which seems to me very seldom to be correctly viewed, and which has given rise to misunderstandings that have had serious consequences for psychology.

We have seen above how Galileian concepts separated the previously undifferentiated questions of the historical course of events, on one side, and of the laws of events on the other. They renounced in systematic problems the immediate reference to the historic-geographic datum. That the procedure instituted does not, as might at first appear, contradict the "empirical" tendency toward the comprehension of the full reality may already be clear from our last consideration: the Aristotelian immediate relation to the historically regular and its average really means giving up the attempt to understand the particular, always situation-conditioned event. Only when this *immediate* relation is completely abandoned, when the place of historic-geographic constancy is taken by the position of the particular in the whole situation, and when (as in experimental method) it is just the same whether the situation is frequent and permanent or

[12]It is impossible here to go more fully into the problem of induction. (Cf. Lewin, 7.)

rare and transitory, only then does it become possible to undertake the task of understanding the real, always ultimately unique, event.

4. *The Meaning of the Process Differential.* Methodologically there may seem to result here another theoretical difficulty which can perhaps be better elucidated by a simple example than by general discussion. In order that the essentials may be more easily seen, I choose an example not from familiar physics but from problematical psychology. If one attempt to trace the behavior of a child to psychical *field forces* among other things—the justification for this thesis is not here under discussion—the following objection might easily be raised: a child stands before two attractive objects (say a toy, T, and a piece of chocolate, C) which are in different places (see Figure 1). According to this hypothesis, then, there exist field

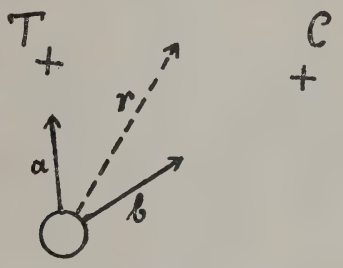

FIGURE 1

forces in these directions (a and b). The proportional strength of the forces is indifferent, and it does not matter whether the physical law of the parallelogram of forces is applicable to psychical field forces or not. So far, then, as a resultant of these two forces is formed, it must take a direction (r) which leads neither to T nor to C. The child would then, so one might easily conclude according to this theory, reach neither T nor C.[13]

In reality such a conclusion would be too hasty, for even if the vector should have the direction r at the moment of starting, that does not mean that the actual process permanently retains this direction. Instead, the *whole situation changes with the process,* thus changing also the vectors that at each moment determine the dynamics, in both their strength and direction. Even if one assume

[13] I am neglecting here the possibility that one of the field forces entirely disappear.

the parallelogram of forces and in addition a constant internal situation in the child, the actual process, because of this changing in the situation, will always finally bring the child to one or the other of the attractive objects (Figure 2).[14]

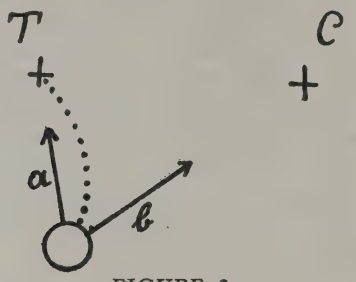

FIGURE 2

What I would like to exhibit by this example is this: If one try to deduce the dynamics of a process, particularly the vectors which direct it, from the actual event, one is compelled to resort to process differentials. In our example, one can regard only the process of the first moment, not the whole course, as the immediate expression of the vector present in the beginning situation.

The well-known fact that all, or at least most, physical laws are differential laws (11) does not seem to me, as is often supposed, to prove that physics endeavors to analyze everything into the smallest "elements" and to consider these elements in the most perfect possible isolation. It proceeds rather from the circumstance that physics since Galileo no longer regards the *historic course of a process* as the immediate expression of the vectors determinative of its dynamics. For Aristotle, the fact that the movement showed a certain total course was proof of the existence of a tendency to that course, for example, toward a perfect circular movement. Galileian concepts, on the contrary, even in the course of a particular process, separate the quasi-historical from the factors determining the dynamics. They refer to the whole situation in its full concrete individuality, to the state of the situation at every moment of time.

Further, for Galileian concepts, the forces, the physical vectors

[14]Even if the distances of the attractive objects and the strength of their attractions were equal, the resulting conflict situation would lead to the same result, owing to the liability of the equilibrium.

which control the situation, are proved by the resulting process. However, it is valid to exclude the quasi-historical in order to get the "pure" process, and hence necessary to comprehend the type of process by recourse to the process differential, because only in the latter, and hence unmixed, is it expressed. This recourse to the process differential thus arises not, as is usually supposed, from a tendency to reduce all events to their "ultimate elements," but as a not immediately obvious complementary expression of the tendency to derive the dynamics from the relation of the concrete particular to the concrete whole situation and to ascertain the type of event with which this total situation is dynamically related as "purely" and as unmixed with historic factors as possible.

Experimentally also it is important to construct such situations as will actually yield this "pure" event, or at least permit of its conceptual reconstruction.

5. *Methodological.* It remains to examine more closely the logical and methodological consequences of this mode of thought. Since law and individual are no longer antitheses, nothing prevents relying for proof upon historically unusual, rare, and transitory events, such as most physical experiments are. It becomes clear why it is very illuminating, for systematic concepts, to produce such cases, even if not exactly for the sake of their rarity itself.

The tendency to comprehend the actual situation as fully and concretely as possible, even in its individual peculiarities, makes the most precise possible *qualitative* and *quantitative* determination necessary and profitable. But it must not be forgotten that only this task, and not numerical precision for its own sake, gives any point or meaning to "exactness."

Some of the most essential services to knowledge of the quantitative, and in general of the mathematical, mode of representation are (1) the possibility of using *continuous* transitions instead of dichotomies in characterization, thereby greatly refining description, and (2) the fact that with such "functional concepts" it is possible to go from the particular to the general *without losing the particular in the general* and thereby making impossible the return from the general to the particular.

Finally, reference should be made to the method of "approximation" in the description of objects and situations, in which the "continuous," functional mode of thought is manifest.

B. Fundamental Dynamic Concepts in Psychology

The dynamic concepts of psychology today are still thoroughly Aristotelian,[15] and indeed the same internal relations and motives seem to me here displayed, even to the details.

1. *Aristotelian Ideas: Independence of the Situation. Instinct.* In content, which is easiest to exhibit and indeed hardly requires exposition, psychological dynamics agrees most completely with Aristotelian concepts: It is "teleology" in the Aristotelian sense. The traditional mistake of regarding "causal" explanation as an explanation without the use of directed forces has notably retarded the progress of dynamics, since psychological dynamics, like physical, cannot be understood without the use of vector concepts. It is not the fact that directed quantities are employed in psychological dynamics that gives it its Aristotelian character, but the fact that the process is ascribed to vectors connected with the object of investigation, for example, with the particular person and *relatively independent of the situation.*

The concept of instinct in its classical form is perhaps the most striking example of this. The instincts are the sum of those vectors conditioned by "predispositions" which it is thought must be ascribed to an individual. The instincts are determined essentially by finding out what actions occur most frequently or regularly in the *actual life* of the individual or of a group of like individuals. That which is *common* to these frequent acts (e.g., food-getting, fighting, mutual aid) is regarded as the *essence* or essential nature of the processes. Again, completely in the Aristotelian sense, these abstract class concepts are set up as *at once the goal and the cause* of the process. And indeed the instincts obtained in this way, as averages of historical actuality, are regarded as more fundamental the more *abstract* the class concept is, and the more various the cases of which the average is taken. It is thought that in this way, and *only* in this way, those "accidents" which inhere in the particular case and the concrete situation can be overcome. For the aim which still completely dominates the procedure of psychology in large fields is founded upon the effort to *free itself of the connection to specific situations.*

2. *Intrinsic Difficulties and Unlawfulness.* The whole difference between the Aristotelian and Galileian modes of thought becomes

[15]The same holds, incidentally, for biology, which I cannot here especially examine, although I regard psychology in general as a field of biology.

clear as soon as one sees what consequences, for a strict Galileian view of the concept of law, follow from this close and fixed connection of the instinct to the individual "in itself." In that case the instinct (e.g., the maternal) must operate *continually* without interruption; just as the explanation of negativism by the "nature" of the three-year-old child entails for Galileian concepts the consequence that *all* three-year-old children must be negative *the whole day long,* twenty-four hours out of the twenty-four.

The general Aristotelian set of psychology is able to dodge these consequences. It is satisfied, even for proof of the existence of the vectors which should explain the behavior, to depend upon the concept of "regularity." In this way it avoids the necessity of supposing the vector to be existent in every situation. On the basis of the strict concept of law it is possible to disprove the hypothesis, for example, of the existence of a certain instinct by demonstrating its non-existence in given concrete cases. Aristotelian concepts do not have to fear such disproofs, inasmuch as they can answer all references to concrete particular cases by falling back on mere statistical validity.

Of course these concepts are thereby also unable to explain the occurrence of a *particular case,* and by this is meant not the behavior of an abstractly defined "average child," but, for example, the behavior of a certain child at a certain moment.

The Aristotelian bent of psychological dynamics thus implies not only a limitation of explanation to such cases as occur frequently enough to provide a basis for abstracting from the situation, but it leaves literally *any* possibility open in any particular case, even of frequent events.

3. *Attempts at Self-Correction: the "Average" Situation.* The intrinsic difficulties for dynamics which the Aristotelian mode of thought brings with it, viz., the danger of destroying the explanatory value of the theory by the exclusion of the situation, are constantly to be observed in contemporary psychology, and lead to the most singular *hybrid* methods and to attempts to include the concept of the situation *somehow.* This becomes especially clear in the attempts at quantitative determination. When, for example, the question is raised and an attempt made to decide experimentally how the strengths of various drives in rats (perhaps hunger, thirst, sex, and mother love) compare with each other, such a question (which corresponds to asking in physics which is stronger, gravitation or

electromotive force) has meaning only if these vectors are ascribed entirely to the rat and regarded as practically independent of the concrete whole situation, independent of the condition of the rat and its environment at the moment. Such a fixed connection is, of course, ultimately untenable, and one is compelled at least in part to abandon this way of thinking. Thus the first step in this direction consists in taking account of the *momentary condition of the drive* with regard to its state of satiation: The various possible degrees of strength of the several drives are ascertained, and their *maximal* strengths are compared.

It is true, of course, that the Aristotelian attitude is really only slightly ameliorated thereby. The curve expresses the statistical average of a large number of cases, which is not binding for an individual case; and, above all, this mode of thought applies the vector independently of the structure of the situation.

To be sure, it is not denied that the situation essentially determines the instinctive behavior in the actual particular case, but in these problems, as in the question of the child's spontaneous behavior in the baby tests, it is evident that no more is demanded of a law than a behavioral average. The law thus applies to an "average" situation. It is forgotten that there just is no such thing as an "average situation" any more than an average child.

Practically, if not in principle, the reference to the concept of an *"optimal"* situation goes somewhat further. But even here the concrete structure of the situation remains indeterminate: only a maximum of results in a certain direction is required.

In none of these concepts however are the two fundamental faults of the Aristotelian mode of thought eliminated: the vectors determining the dynamics of the process are still attributed *to the isolated object, independently of the concrete whole situation;* and only very slight demands are made upon the *validity of psychological principles* and the comprehension of the concrete actuality of the individual single process.

This holds true even for the concepts immediately concerned with the significance of the situation. As mentioned before, the question at the center of the discussion of the situation is, quite in the Aristotelian sense, how far the situation can "hinder" (or "facilitate"). The situation is even considered as a constant object and the question is discussed: which is more important, heredity or environment? Thus again, on the basis of a concept of situation

gotten by abstraction, a dynamic problem is treated in a form which has none but a statistical historical meaning. The heredity or environment discussion also shows, even in its particulars, how completely these concepts separate object and situation and derive the dynamics from the isolated object itself.

The rôle of the situation in all these concepts may perhaps be best exhibited by reference to certain changes in painting. In mediaeval painting at first there was, in general, no environment, but only an empty (often a golden) background. Even when gradually an "environment" did appear it usually consisted in nothing more than presenting, beside the one person, other persons and objects. Thus the picture was at best an assembling of separate persons in which each had really a separate existence.

Only later did the space itself exist in the painting: it became a whole situation. At the same time this situation as a whole became dominant, and each separate part, so far indeed as separate parts still remain, is what it is, for example, in such an extreme as Rembrandt, only in and through the whole situation.

4. *Beginnings of a Galileian Mode of Thought.* Opposed to these Aristotelian fundamental ideas of dynamics there are now signs in psychology of the *beginnings of a Galileian mode of thought.* In this respect the concepts of sensory psychology are farthest advanced.

At first, even in sensory psychology, explanations referred to isolated single perceptions, even to single isolated elements of these perceptions. The developments of recent years have brought about, first slowly but then more radically, a revolution in the fundamental dynamic ideas by showing that the dynamics of the processes are to be deduced, not from the single elements of the perception, but from its *whole structure.* For it is impossible by a consideration of the elements to define what is meant by "figure" in the broader sense of the word. Rather, the whole dynamics of sensory psychological processes depend upon the "ground" (12) and beyond it upon the structure of the whole surrounding field. The dynamics of perception are not to be understood by the abstract Aristotelian method of excluding all fortuitous situations, but—this principle is penetrating today all the fields of sensory psychology—only by *the establishment of a form of definite structure in a definite sort of environment.*

Recently the same fundamental ideas of dynamics have been extended beyond the special field of perception and applied in the fields

of higher mental processes, in the psychology of instinct, will, emotion, expression, and in genetic psychology. The sterility, for example, of the always circular discussion of heredity or environment and the impossibility of carrying through the division, based upon this discussion, of the characteristics of the individual, begin to show that there is something radically wrong with their fundamental assumptions. A mode of thought is becoming evident, even though only gradually, which, corresponding somewhat to the biological concept of phenotype and genotype, tries to determine the predisposition, not by excluding so far as possible the influence of the environment, but by accepting in the concept of disposition its necessary reference to a group of concretely defined situations.

Thus in the psychological fields most fundamental to the whole behavior of living things the transition seems inevitable to a Galileian view of dynamics, which does not derive all its vectors from single isolated objects, but from the mutual relations of the factors in the concrete whole situation, that is, essentially, from the momentary condition of the individual and the structure of the psychological situation. *The dynamics of the processes is always to be derived from the relation of the concrete individual to the concrete situation,* and, so far as internal forces are concerned, from the mutual relations of the various functional systems that make up the individual.

The carrying out of this principle requires, to be sure, the completion of a task that at present is only begun: namely, the providing of a workable representation of a *concrete psychological situation* according to its individual characteristics and its associated functional properties, and of the concrete structure of the psychological *person* and its "internal" dynamic facts. Perhaps the circumstance that a technique for such a *concrete representation,* not simply of the physical but of the psychological situation, cannot be accomplished without the help of topology, the youngest branch of mathematics, has contributed to keeping psychological dynamics, in the most important fields of psychology, in the Aristotelian mode of thought. But more important than these "technical" questions may be the general substantial and "philosophical" presuppositions: too meager scientific courage in the question of the *lawfulness* of the psychical, too slight demands upon the validity of psychological laws, and the tendency, which goes hand in hand with this leaning toward mere regularity, to specifically *historic-geographic concepts.*

The accidents of historical processes are not overcome by excluding the changing situations from systematic consideration, but only by taking the fullest account of the individual nature of the concrete case. *It depends upon keeping in mind that general validity of the law and concreteness of the individual case are not antitheses, and that reference to the totality of the concrete whole situation must take the place of reference to the largest possible historical collection of frequent repetitions.* This means methodologically that the importance of a case, and its validity as proof, cannot be evaluated by the frequency of its occurrence. Finally, it means for psychology, as it did for physics, a transition from an abstract classificatory procedure to an essentially concrete constructive method.

That psychology at present is not far from the time when the dominance of Aristotelian concepts will be replaced by that of the Galileian mode of thought seems to me indicated also by a more external question of psychological investigation.

It is one of the characteristic signs of the "speculative" early stage of all sciences that "schools," representative of different "systems," oppose each other in a way and to an extent that is unknown, for example, in contemporary physics. When a difference of hypotheses occurs in contemporary physics there still remains a common basis that is foreign to the schools of the speculative stage. This is only an external sign of the fact that the concepts of that field have introduced a method that permits step-by-step approximation to understanding. Thereby results a continuous progress of the science which is constantly more narrowly limiting the consequences for the whole structure of differences between various physical theories.

There seems to me much to indicate that even the development of the schools in contemporary psychology is bringing about a transition to a similar sort of constant development, not only in sensory psychology but throughout the entire field.

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LE CONFLIT ENTRE LES MANIÈRES DE PENSER ARISTOTÉLICIENNE ET GALILÉENNE DANS LA PSYCHOLOGIE CONTEMPORAINE

(Résumé)

Les conflits de la psychologie contemporaine montrent dans leur structure conceptuelle un rapport étroit avec la transition de la manière de penser Aristotélicienne-médiévale à la manière de penser de la physique post-Galiléenne moderne. Il est on ne peut plus important pour les problèmes urgents de la psychologie qu'on rend clair ce parallèle.

C'était une condition nécessaire à la révolution dans la formation conceptuelle de la physique que la destruction de la *séparation* "anthropomorphique" des sphères céleste et terrestre. Dans la psychologie c'est la destruction du mur entre le normal et l'anormal, l'homme et l'animal, l'enfant et l'adulte.

Cette réduction à l'homogénéité n'est pas un nivellement des différences individuelles. L'emploi des aides mathématiques par la physique Galiléenne ne signifie pas une tendance à l'*abstrait,* mais précisément une lutte contre l'abstraction soustractive Aristotélicenne des différences individuelles. Pour la physique Aristotélicienne, le cas individuel était *fortuit,* et tout ce qui est valide, c'est ce qui se passe fréquemment ou toujours dans la vraie succession historique des événements du monde, un peu comme dans la psychologie statistique contemporaine de l'enfant.

La moyenne abstraite, la "classe," est en même temps que "l'essence," déterminative de la dynamique ("téléologique"). Le processus dépend de

forces attribuées aux objects isolés, essentiellement sans égard à ce qui les environne. Des idées tout à fait semblables de la dynamique dominent la psychologie contemporaine de l'impulsion et de l'expression.

Au lieu de se rapporter à la fréquence de l'occurrence historique et du nivellement des différences individuelles, la physique *Galiléenne* conçoit même l'événement individuel unique comme valide, et dérive la dynamique du processus de la relation de l'individu concrete à l'ensemble du milieu concret où il se trouve à chaque instant. De ces considérations il est possible d'obtenir des renseignements essentials sur le changement dans les concepts fondamentaux dynamiques de la psychologie, surtout sur des questions fondamentales de la théorie de la Gestalt et du problème de la téléologie.

LEWIN

DER KAMPF ZWISCHEN DER ARISTOTELISCHEN UND DER GALILEISCHEN GEDANKENSWEISE IN DER GEGENWÄRTIGEN PSYCHOLOGIE

(Referat)

Die Kämpfe der gegenwärtigen Psychologie zeigen ihrer begrifflichen Struktur nach eine enge Verwandtschaft mit dem Uebergang vom *aristotelisch*-mittelalterlichen Denken zur Denkweise der modernen *nachgalileischen* Physik. Sich dieser Parallele klar zu werden, ist auch für die aktuellen Probleme der Psychologie von wesentlicher Bedeutung.

Eine Voraussetzung des Umschwungs der Begriffsbildung war in der Physik die Ueberwindung der "anthropomorphen" Trennung der 'himmlischen" und "iridischen" Sphären, in der Psychologie ist es das Aufheben der Scheidewand zwischen normal und anormal, Mensch und Tier, Kind und Erwachsenen.

Diese *Homogenisierung* ist *keine Nivellierung* der individuellen Unterschiede. Die Benutzung der mathematischen Hilfsmittel der galileischen Physik bedeutet nicht einen Uebergang zum *Abstrakten,* sondern gerade einen Kampf gegen die aristotelische substraktive Abstraktion der individuellen Unterschiede. Für die aristotelische Physik gilt das *Individuelle* als *zufällig* und nur das, was im tatsächlichen historischen Ablauf der Welt häufig oder immer auftritt als gesetzlich, ähnlich etwa wie in der gegenwärtigen statistischen Kinderpsychologie. Der abstrakte Durchschnitt, die "Klasse," wird als "Wesen" zugleich bestimmend für die (teleologische) *Dynamik.* Das Geschehen geht auf Kräfte zurück, die den isolierten Gegenständen im wesentlichen ohne Rücksicht auf ihre Umgebung zukommen. Ganz ähnliche dynamische Vorstellungen beherrschen die gegenwärtige Trieb- und Ausdruckspsychologie.

An Stelle der Bezugnahme auf die Häufigkeit des historischen Vorkommens und der Nivellierung der individuellen Unterschiede begreift die *galileische* Physik auch das *individuell* Einmalige als gesetzlich und leitet die Dynamik des Geschehens aus der Beziehung des konkreten Individuums zum Ganzen der konkreten Umwelt her, in der es momentan steht. Von hier aus lassen sich wesentliche Einsichten in die Wandlung der dynamischen Grundbegriffe der Psychologie, insbesondere in einige Grundfragen der Gestalttheorie und das Problem der Teleologie gewinnen.

LEWIN

THRESHOLDS OF ILLUMINATION FOR THE VISUAL DISCRIMINATION OF DIRECTION OF MOVEMENT AND FOR THE DISCRIMINATION OF DISCRETENESS*

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INTRODUCTION

The present paper offers two contributions: (*a*) a comparison of the thresholds of illumination for the visual discrimination of movement and for the discrimination of direction of line; and (*b*) a consideration of Hecht and Wolf's test of acuity. These authors (3) sought to determine acuity in the honey bee not by requiring the bee to make a discrimination on the basis of the discreteness of stationary visual stimuli but by requiring the bee to respond on the basis of the direction in which the visual stimulus pattern was moved. Conventionally the term acuity refers to a definite type of behavior problem, viz., the discrimination of discreteness in a stationary stimulus where the limits of discrimination are measured in terms of the visual angles subtended by the stimuli discriminated. The discrimination of the direction of movement is, from the standpoint of the task given the subject, quite a different problem. If results secured by the two methods are significantly different, the methods are not equally adequate for the measurement of acuity.

The general method of experimentation followed by Hecht and Wolf was as follows: Diffused light of a variable intensity was cast from below upon an inclined glass plate upon which were alternate black and white stripes of equal width. Above this glass and parallel to it was a uniform glass plate upon which the bees crept. When the bee was moving upward on this plate the grating was moved with a sharp motion to the right or left. When the stimulus moved left, the bee oriented sharply to the right, and vice versa. Thirteen gratings were used, the widest stripes being 19 mm. and the narrowest 0.318 mm. wide. For each grating that intensity of illumination

*Accepted for publication by Carl Murchison of the Editorial Board.

was determined at which the bee just responded to the direction of movement. The results are plotted with the log of intensity (in millilamberts) on the base line and the reciprocals of the visual angles subtended at the bee's eye by the stripes (multiplied by 10^4) on the vertical line. The curve so secured is sigmoid, as is also the well-known König curve showing the relation between light intensity and the discrimination of discreteness of stationary visual stimuli (visual acuity) by human subjects. A comparison of the data on bees with that on human subjects (König) leads the authors to conclude that the human subjects "can resolve the environment about 100 times better than a bee can" (3, p. 739).

In the present paper we are not concerned with Hecht's theory but with his proposed test of acuity. The mere fact of a sigmoid relationship between the widths of the visual lines and the log of the intensities of light which will just enable the bee to discriminate the direction of the lines' movement seems to us no proof that the method is one for the adequate measure of acuity. Presumably any factor which varies with the visual angle subtended by the stimulus in some way influences the discriminatory responses of the subject. Hecht and Wolf have now shown for the bee, and the present paper shows for human subjects, that the degree of illumination required to extinguish the discrimination of direction of movement is a function of the visual angle subtended by the stimulus. (It seems to us that the above description of the problem is much closer to the facts than is Hecht and Wolf's use of the term acuity. Their bees were required to respond to the direction of movement of the visual stimulus and not to the discreteness of the stimulus.)

In seeking to determine whether or not Hecht and Wolf's method of measuring acuity is adequate, we have determined for human subjects the intensity thresholds for the discrimination of direction of visual movement and for the discrimination of the direction of visual lines using the same set of lines in the two determinations. The results secured by the two methods are very different one from the other and lead us to the conclusion that the method proposed by Hecht and Wolf does not adequately measure acuity.

Apparatus and Method

The apparatus used in the experiment was essentially that of Hecht and Wolf adapted for use with human subjects. In determining the threshold of light intensity for the discrimination of direction of

movement, the subject regarded an illuminated 10x10-cm. surface composed of alternating light and dark bars. At a signal the experimenter moved this grating either to the right or left. After the movement the subject responded orally, indicating the direction of movement. The intensities of illumination were determined at which the correctness of response to the direction of movement was of a chance order. This determination was made for each of seven gratings differing one from the other in the width of bars. (See Table 1.) In determining the threshold of light intensity for the discrimination of lines, the same gratings were used. They were inserted in the apparatus with the bars either vertical or horizontal, and the subject was instructed to respond "vertical" or "horizontal." The threshold for each grating was taken as that illumination at which the subject failed to discriminate the direction of the bars.

The details of the apparatus are shown in Figure 1. This drawing represents a top view of the assembly. Illumination is provided by means of a 1000-watt Mazda lamp, *L*, in a ventilated light-box, *LB*. This box is 85 cm. long, 60 cm. high, and 50 cm. wide. The light-box projects through the wall of the darkroom, *W*. In the front end of the box, at a distance of 30 cm. from the light, is a double convex lens, *L'*, having a diameter of 15 cm. The lens is set in a screen. The rays of light from *L* are brought to a focus slightly in front of the forward aperture (2½ x 1½ cm.) of the light-box. Back of this aperture is a Wratten neutral tint filter, *F*. (The filter used has a density of 3. It was used with gratings 5 to 7. No filter was used with gratings 1 to 4.) In front of the aperture is a balancing wedge, *BW*. A calibrated neutral tint wedge, *NW*, in a holder is placed in front of *BW*. *NW* can be moved across the face of *BW*, its position determined at a given threshold, and the intensity calculated with a calibration chart. The rays of light which pass through this series of filters illuminate a milk-glass plate, *MG*. This illumination is distributed over a 10 x 10-cm. opening in the wall-board screen, *S*. *MG* is not set in the screen, but is about 0.7 cm. behind the opening. Gratings, *G*, are slipped into the space between the screen and the milk-glass. The space between the screen, *S*, and the light-box, *LB*, is doubly draped with black velvet. A further safeguard against escaping light is the presence of a light-trap, *LT*. This may be schematically described as a square, blackened box, the open top of which faces the calibrated wedge, the bottom being formed by the milk-glass plate. A movable stand with a chin-rest and a stereoscope hood, with the lenses removed, is provided for the subjects.

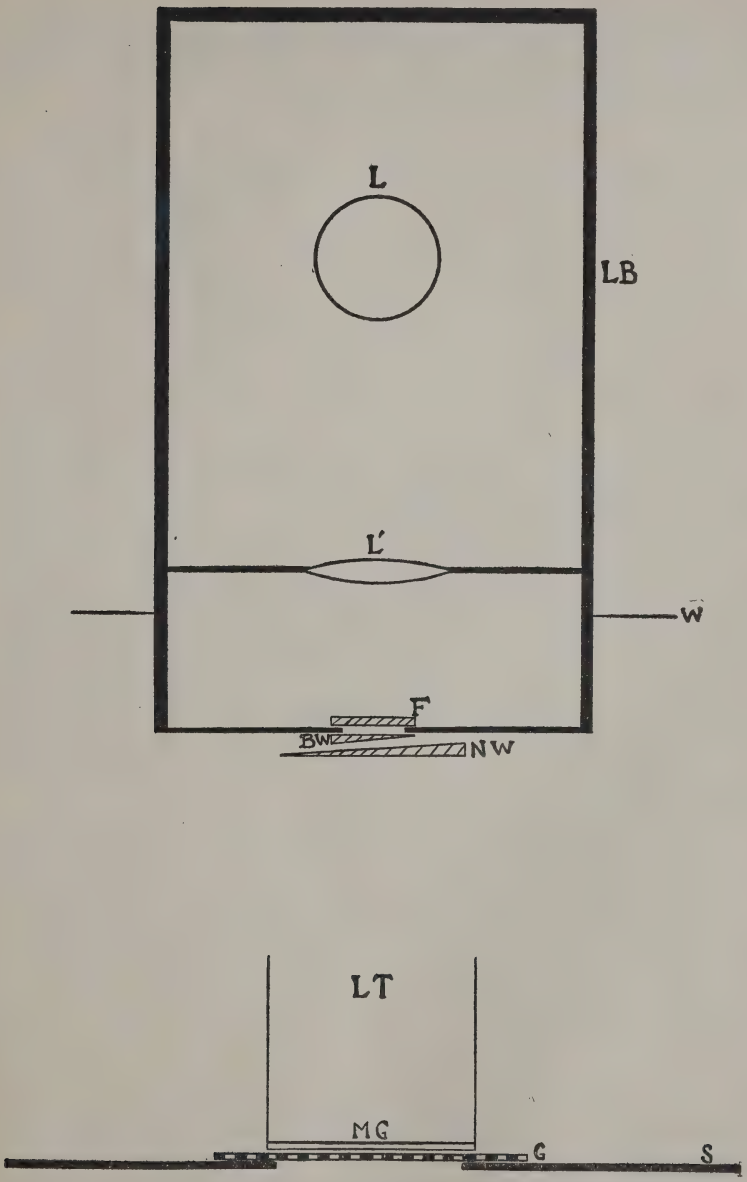

FIGURE 1

GROUND PLAN OF THE APPARATUS

L, a 1000-watt lamp; *L'*, lens; *F*, Wratten neutral tint filter; *BW*, balancing wedge; *NW*, calibrated neutral tint wedge; *LB*, light-box; *W*, wall of dark-room; *LT*, light-trap; *MG*, milk-glass plate; *G*, grating; and *S*, wall-board screen.

Seven widths of bars were used. The wider ones were made by sticking opaque gummed paper on 20 x 10-cm. glass plates, and the finer ones were engraved by Max Levy & Co., Philadelphia. Table 1 gives the width of the bars on each grating.

TABLE 1

Grating	Width of bar in mm.
1	0.318
2	0.423
3	0.635
4	1.27
5	4.23
6	6.92
7	10.30

Since Freeman (1) has shown that variations in fixation distance produce anomalous results in acuity determinations, each subject was kept at a constant distance from the gratings. This means that for each subject the fixation distance for the fine bars was the same as for the wider bars. This distance was determined on the basis of the subject's reaction to grating No. 1. Using grating No. 1 and the highest illumination (37.8 millilamberts[1]) which our apparatus afforded, the subject's discrimination threshold for direction of movement was determined by varying his distance from the grating. Once this distance was determined for a given subject, the same distance was used for that subject with all other gratings. (With these other gratings thresholds were determined by varying the light intensity directly and not by varying the distance.)

In determining the threshold for direction of movement the subject was dark-adapted for 4 minutes, except in the case of gratings 5, 6, and 7 where a 15-minute interval was used. The apparatus was adjusted to give an intensity of light well above the threshold for the grating used. The proper grating was inserted (always with the lines vertical), and the experimenter got ready to move the pattern plate sharply, but not rapidly, to the right or left. At the close of the dark-adaptation period the experimenter said "all right," and the subject raised his eyes and regarded the now illuminated area. Two seconds later, at the "ready" signal, he prepared for a response, and a second later, at the "now" signal, the grating was

[1]All intensities were determined photometrically.

moved. The subject reported verbally the direction of movement as either right or left. Early in the experiments the above-mentioned signals were timed with a metronome, but later this was abandoned when the experimenter became more practiced.

A similar procedure was used in the experiments for the determination of thresholds under stationary conditions. The distance of each subject from the gratings was the same as in the previous experiment. The same periods of dark- and light-adaptation were allowed as in the previous experiment, and the same signals were used as before. The gratings were inserted with the bars either horizontal or vertical, the positions being varied in a chance order and the gratings being stationary when they stimulated the subjects. Immediately after the "now" signal the subject was required to report "vertical" or "horizontal." The reason for this was as follows. Under conditions of movement the factors eliciting a discrimination ceased with the end of the movement. With the bars stationary, however, the stimulating conditions remained the same. It was thus necessary to limit the time of discrimination in order to make the temporal aspects of the two tests similar. Although in both experiments the work with each grating began with an illumination well above the threshold, the determination of each threshold was made both by decreases and by increases of the light intensities.

Determinations of the thresholds for gratings 2, 3, and 4 were made at one sitting, and the thresholds for gratings 5, 6, and 7 were made at another. With the former set of gratings, and with grating 1, the 4-minute dark-adaptation period was considered sufficient since the light intensities with these fine bars is relatively high. In the case of the wide bars, gratings 5 to 7, a 15-minute period of dark-adaptation was considered necessary. After these intervals the subject was instructed to look at the illuminated area. This was done for the purpose of adapting to the intensity of the area. The periods between stimulations were not over 4 seconds long, and the period of fixation of the lighted area before the "now" signal was 3 seconds long, so that the degree of adaptation was relatively constant.

Results and Discussion

Five subjects were used in these experiments. The results for each of these subjects for direction of movement are summarized in Table 2. The results obtained for direction of stationary lines are

given in Table 3. In these tables the second columns give the reciprocals of the visual angles subtended by the seven stimuli, and the third columns give the intensities at which the discrimination of line direction or movement direction was liminal.

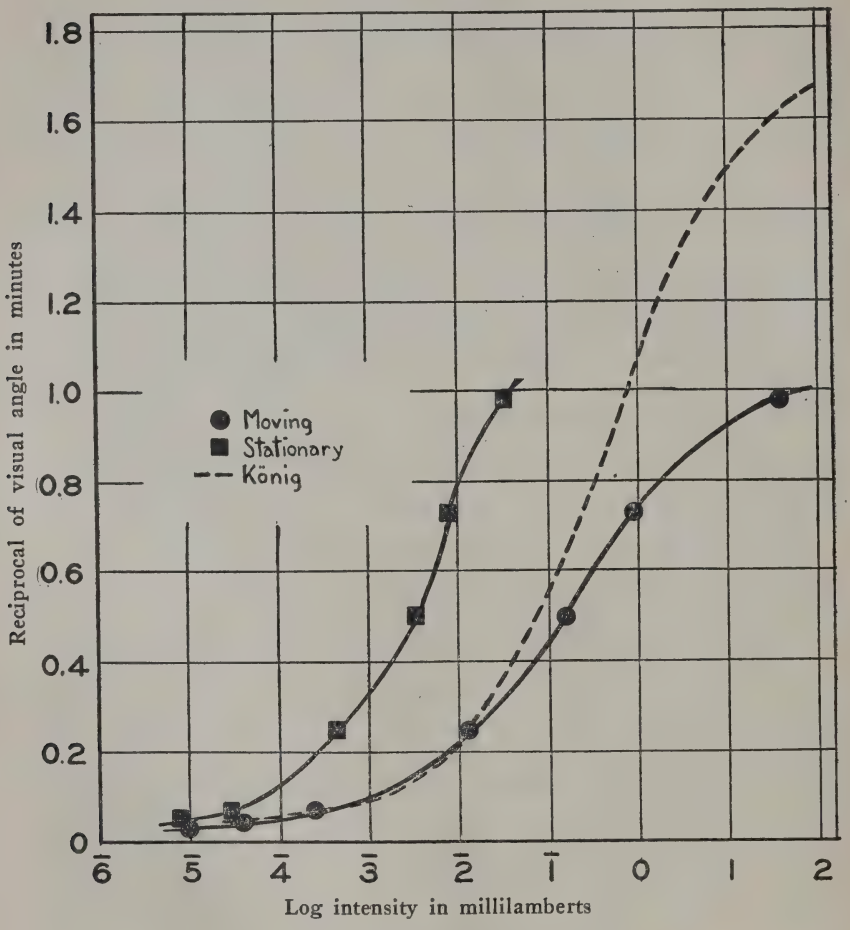

FIGURE 2

THRESHOLDS OF ILLUMINATION FOR THE VISUAL DISCRIMINATION OF DIRECTION OF MOVEMENT AND FOR THE DISCRIMINATION OF DISCRETENESS

The dotted curve represents König's data on acuity.
Reciprocal of visual angle in minutes. Log intensity in millilamberts.

In order to facilitate comparison, the averaged data of the five subjects for both experiments are presented in Table 4 and Figure 2. The reciprocals of visual angles are averaged for the five subjects, and the intensities are the logarithmic averages of the corresponding illuminations. It is clear from a survey of Tables 2 and 3 that the differences in the curves of Figure 2 are real differences. For example, let us compare the threshold illuminations for the same visual angles obtained under stationary as opposed to moving conditions.

TABLE 2

THRESHOLD INTENSITIES FOR THE DISCRIMINATION OF MOVEMENT

Subject	Reciprocal of visual angle	Intensity
C	1.11	37.8
	0.83	1.08
	0.56	0.26
	0.27	0.0041
	0.083	0.00045
	0.051	0.000095
	0.034	0.0000063
Go	0.80	37.8
	0.60	2.17
	0.40	0.58
	0.21	0.079
	0.062	0.00029
	0.038	0.000017
	0.025	0.0000083
L	1.04	37.8
	0.80	0.62
	0.52	0.15
	0.27	0.022
	0.08	0.00025
	0.048	0.00007
	0.036	0.000017
H	0.98	37.8
	0.72	0.27
	0.49	0.043
	0.24	0.005
	0.07	0.00013
	0.044	0.000014
	0.029	0.0000036
O	0.98	37.8
	0.72	1.73
	0.49	0.114
	0.24	0.009
	0.07	0.00029
	0.044	0.000045
	0.029	0.000021

Subject L's threshold for direction of movement with grating No. 1 is 37.8 millilamberts, while with the same grating the limen for direction of line is 0.031 millilamberts. It appears, then, that with a stationary visual field the same degree of discrimination can be attained with only one thousandth of the illumination required for a moving field. In the averaged results as given in Figure 2 we see that this ratio also exists for grating No. 1. There is between the illuminations at the highest value of the reciprocal of the visual angle

TABLE 3

THRESHOLD INTENSITIES FOR THE DISCRIMINATION OF STATIONARY LINE DIRECTION

Subject	Reciprocal of visual angle	Intensity
C	1.11	0.023
	0.83	0.0057
	0.56	0.00091
	0.27	0.00041
	0.083	0.00004
	0.051	0.0000055
Go	0.80	0.014
	0.60	0.0043
	0.40	0.003
	0.21	0.00031
	0.062	0.000017
	0.038	0.0000057
L	1.04	0.031
	0.80	0.0045
	0.52	0.003
	0.27	0.0014
	0.08	0.000055
	0.048	0.000026
H	0.98	0.045
	0.72	0.03
	0.49	0.012
	0.24	0.00031
	0.07	0.000021
	0.044	0.0000057
O	0.98	0.099
	0.72	0.009
	0.49	0.0045
	0.24	0.00022
	0.07	0.000019
	0.044	0.0000055

TABLE 4

COMPARISON OF AVERAGE DATA FOR STATIONARY AND MOVING CONDITIONS

Reciprocal of visual angle	Moving Log I	Stationary Log I
0.98	1.58	2.53
0.73	1.96	$\bar{3}$.90
0.49	$\bar{1}$.20	$\bar{3}$.53
0.25	$\bar{2}$.10	$\bar{4}$.62
0.073	$\bar{4}$.42	$\bar{5}$.43
0.045	$\bar{5}$.57	$\bar{6}$.88
0.031	$\bar{6}$.97	

(0.98) a difference of approximately 3 logarithmic units. With grating No. 3, subject L has a threshold of 0.003 millilamberts for the stationary condition and a threshold of 0.15 millilamberts for the condition of movement. This ratio of 1 to 50 is also present in the averaged data where there is a difference of about 1.67 logarithmic units.

With these examples in mind we can feel certain that the great differences found in our averaged curves are real. Approximately the same ratios of threshold illuminations for the two conditions which are found in the averaged data can be found in the data for any subject.

One fact stands out predominantly. There is a great difference in the illumination necessary for the two types of discrimination investigated. We hesitate to make any comparisons between our thresholds secured with human subjects and those obtained by Hecht and Wolf with bees. Such a comparison, although of doubtful validity, would show that the maximum fineness of discrimination for a human being under conditions of movement is about 60 times that of the bee. This ratio does not hold for the whole range of the curve, but further comparison is even less valid, inasmuch as the determined range of threshold illuminations for the bee is very much smaller than for the human subject. In this, as in many other cases where human and infra-human performances are compared, the control of the human experimental conditions is so much the better of the two that the infra-human subject's capacity is often rated too low.

In addition to the curves constructed from the data of our own experiments, we have given in Figure 2 a graphical presentation of König's (2, 4) data on acuity. This is done because of the fact that König's curve has played such an important rôle in the formulations of theories of acuity. It is to be noted that this curve falls between our two curves and extends to a greater value of the reciprocal of the visual angle. The intermediate position of this curve is probably due to the fact that König used a different type of test object, that his angular unit was more nearly 1 1-3 minutes than 1 minute, and that we know little of his criterion of discrimination.

Certain comments can be made upon the physiological processes underlying the performances represented in our two curves. Let us first take the case of the stationary field. The retinal elements stimulated by the illuminated bars are in a state of light-adaptation, while those between are in a state of relative dark-adaptation. The impulses in the optic nerve are the result of processes in the light-adapted elements, and the impulses continuing during the discrimination period have their initiation in the processes going on in spatially discrete retinal areas. Since this is so, we can say that the discrimination of direction of lines is a spatially determined process.

On the other hand, the discrimination of direction of movement is a temporally determined process. In the movement situation we find an illuminated bar stimulating a given retinal element for a short time, followed by a short period of no stimulation. After this latter period there comes another period of increased stimulation when another illuminated bar stimulates the element. This increased stimulation is to some extent a result of the slight degree of dark-adaptation (hence increased sensitivity) produced in the previous interval of no stimulation. In addition to this, we know that the velocity of the dark process is less than that of the light process. Since this is so, it is certain that when a dark bar passes a retinal organ that organ is still in a fair state of light-adaptation. It becomes clear that at any moment in the movement a given light-adapted retinal area is not adjacent to a completely dark-adapted area, but rather to an area in whose fibers the impulses are starting to decline in frequency. This decline lasts only until the area is again stimulated by an illuminated bar.

These comments seem to show that the response elicited under conditions of movement is not initiated by two spatially distinct processes but rather by a single periodic process. In this respect the activity

closely resembles that found in the flicker situation. The discrimination of direction of movement varies with the conditions determining the periodicity of the process, and the width of bars on the gratings is one such determining condition.

Summary

An apparatus, patterned after that used by Hecht and Wolf with bees, was used with human subjects in order to determine the thresholds of illumination necessary for the discrimination of direction of movement and for the discrimination of direction of lines. The results show a marked difference in the illuminations required for the two responses. At the highest reciprocal of the visual angle tested, only about one-thousandth of the illumination required for the movement discrimination is needed for that under stationary conditions. It is suggested that the movement situation elicits a temporally controlled response while the stationary situation elicits a spatially controlled response. In discussing the experiment by Hecht and Wolf on the visual acuity of the honey bee, the authors point out that the bees were discriminating on the basis of direction of movement rather than on the basis of the discreteness of stationary visual stimuli which is the typical acuity situation.

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LES SEUILS D'ILLUMINATION POUR LA DISCRIMINATION VISUELLE DE LA DIRECTION DU MOUVEMENT ET POUR LA DISCRIMINATION DE LA DISCONTINUITÉ

(Résumé)

Un appareil, basé sur celui employé par Hecht et Wolf avec les abeilles, a été employé avec des sujets humains pour déterminer les seuils d'illumination nécessaires pour la discrimination de la direction du mouvement et pour la discrimination de la direction des lignes, Les résultats montrent une

grande différence dans les illuminations nécessaires pour les deux réponses. Au réciproque le plus élevé de l'angle visuel testé il ne faut qu'environ le millième de l'illumination nécessaire pour la discrimination du mouvement pour celle des situations sans mouvement. On suggère que la situation de mouvement cause une réponse contrôlée temporellement tandis que la situation sans mouvement cause une réponse contrôlée spatialement. En discutant l'expérience par Hecht et Wolf sur l'acuité visuelle de l'abeille, les auteurs montrent que les abeilles discriminaient sur la base de la direction du mouvement plutôt que sur la base de la discontinuité des stimuli visuels sans mouvement, ce qui est la situation typique de l'acuité.

GRAHAM ET HUNTER

DIE SCHWELLEN DER BELEUCHTUNG FÜR VISUELLE UNTERSCHEIDUNG DER BEWEGUNGSRICHTUNG UND FÜR UNTERSCHEIDUNG DER ABGESONDERTHEIT ('DISCRETENESS')

(Referat)

Ein Apparat nach dem Muster des von Helm und Wolf an Bienen gebrauchten wurde an menschlichen Versuchspersonen gebraucht, um die Schwellen der zur Unterscheidung der Bewegungsrichtung und zur Unterscheidung der Linienrichtung nötigen Beleuchtung festzustellen. Die Ergebnisse erwiesen einen starken Unterschied zwischen den zu den zwei Erwiderungen nötigen Beleuchtungen. Bei dem höchsten reziproken Wert des untersuchten visuellen Winkels ist zur Unterscheidung bei stationärem Zustand nur etwa ein Tausendstel der Beleuchtung notwendig, die zur Unterscheidung der Bewegungsrichtung nötig ist. Es wird angedeutet, dass die Bewegungssituation eine zeitlich kontrollierte, die stationäre aber eine räumlich kontrollierte Entgegnung hervorruft. In dem sie die Untersuchung von Hecht und Wolf an der Sehschärfe der Honigbiene besprechen, machen die Verfasser darauf aufmerksam, dass die Bienen viel mehr auf Grund der Bewegungsrichtung unterscheiden wie auf Grund der Abgesondertheit der stationären visuellen Reize, welche die typische Gelegenheit zur Akuität bilden.

GRAHAM UND HUNTER

BRIGHTNESS CONTRAST AND HEYMANS' LAW*

From the Psychological Laboratory of the University of Virginia

FRANK ARTHUR GELDARD

The essential nature of the visual contrast phenomenon is described by the statement that stimulation of one retinal area changes, in some manner and to some degree, the sensitivity of other parts of the retina, the greatest alteration being effected, presumably, in those regions lying contiguous to the area stimulated. There are to be found in the literature of visual science many hypotheses concerning the process by which this transfer is brought about; they range from the reasonable to the ridiculous. It might be expected that we should find ourselves far beyond the hypothecating stage in this matter. A large majority of visual experiments have inherent in their procedures the contrast situation; few experimenters are able to avoid it, even if they would. We have dealt with patches of light and color as though they existed in isolation rather than in relation to visible and effective backgrounds. Rarely is it possible, or desirable for that matter, to deal with visual stimuli that fill, in a homogeneous manner, all visual space. Ordinarily, to be sure, it has been found unnecessary to center attention upon the contrast effect; even considered as a source of experimental error it is not unusual to find "light spot" stimulation regarded as playing the very minor rôle of altering in an insignificant manner the "general adaptation level" of the eye. It must be borne in mind, however, that the conditions for contrast are present wherever there is given simultaneous differential stimulation. Owing to the generality of the contrast situation, we may very well expect to find in the literature the reports of many kinds of work that will throw light on the contrast phenomenon, though the experimenter involved may have intended in many instances the solution of a seemingly different problem.

It would seem then that to gain a clear conception of the nature of contrast it is necessary to raise certain questions which are couched in terms of part-stimulation of the retina and the effect of this upon

*Accepted for publication by Carl Murchison of the Editorial Board and received in the Editorial Office, July 22, 1930.

the sensitivity of neighboring retinal areas. A consideration of the literature assures one that this fundamental problem has not been the victim of total neglect, however unsatisfactory may be the solutions there proposed. Various investigators—Cobb and Geissler (2), Ives (9), Karrer and Tyndall (10), Emerson and Martin (3), Martin (11), to mention only a few—have produced evidences that lead to the conclusion that stimulation of one part of the retina serves to "sensitize" to a slight degree other areas. Particularly has this been demonstrated for the macula-periphery relation, where stimulation of the periphery apparently raises the sensitivity of the macular region. This fact seems to have been so firmly established as to have provided a basis for the improvement of photometrical technique. A case in point is the use by Martin (11) of a peripheral surround to enhance brightness contrast in central vision, the principle having been employed in a contrast photometer of his own device.

Controverting this evidence for a retinal transfer of the type described stands Heymans' recently revived law of central inhibition. As enunciated by Heymans (8) in 1899, the law expresses a relation between the effectiveness of simultaneously presented stimuli and their intensities. To translate this into terms of retinal stimulation, the law states a relation between the sensitivities of two separate sensory areas as conditioned by the mutual effect the stimulation of each has upon the other. Heymans' statement of the law is that the inhibitory power of a stimulus, measured by the intensity of a second stimulus which it can just completely inhibit, is proportional to its intensity. Heymans found the law to hold not only for vision, but for taste, sound, and pressure as well. In vision the law, if well-founded, means that stimulation of one local area of the retina exerts an influence upon other parts, not in the direction of sensitizing them, as others have claimed, but rather to decrease their sensitivity, i.e., to raise their stimulus and differential thresholds. Heymans, to be sure, did not regard this transfer as being effected in the retina, but found it necessary to identify this condition with an inhibition operating in the central nervous system. Theories as to the seat of the process need not concern us here; for the moment the facts are more interesting and profitable.

Renewed interest in Heymans' Law was evinced not long ago when L. T. Spencer (13) tested its applicability to the total bodily behavior of animals in response to visual stimuli. For five white rats he was able to demonstrate that "the intensity of illumination to

which the rats just failed in responding with 80% accuracy varied in direct proportion with the intensity of a second light-stimulus," thus substantiating the law in this field.

J. E. Anderson (1) in 1925 and Spencer (14) in the same year reported experiments in human vision in which they claim to have demonstrated the validity of the law, the former for the alteration by "inhibition" of the differential threshold, the latter for the stimulus threshold. Heymans had performed his experiments and given a statement of the law in the days when it was unnecessary to measure the limens of an entire population in order to lend weight to results. Consequently his observations were taken upon only one individual. The need for repetition of the experiment was obvious. Anderson satisfied this need by measuring the difference thresholds of eight subjects, then noted the raising of these values when another light was introduced to serve as an "inhibitor." Spencer measured, by the tedious psychophysical method of right and wrong cases, the stimulus thresholds of more than 50 subjects. He likewise found heightened threshold values when a second light was presented. Spencer further performed an experiment in which stimulation of one retina was found to raise the threshold for a "passive" stimulus seen by the other eye. Thus the central nature of inhibition appears to have been demonstrated once again.

On the basis of these experiments Spencer has permitted himself to become involved in rather far-reaching theoretical considerations. Papers (15, 16, 17) appearing since the time of the performance of the experimental work urge radical changes in the whole concept of the threshold.

Now something is wrong. Stimulation of a local area of the retina cannot lower the sensitivity in some other part and under precisely the same conditions raise that sensitivity. Either the statement of Heymans, Spencer, and Anderson is incorrect, or that of Martin, Ives, Cobb and Geissler, and others is in error, or both groups are mistaken.

The writer determined to repeat these experiments, using a sufficiently general situation to make possible a ready interpretation of previous work. Certain improvements in procedure were seen at the outset, and an apparatus was constructed which employed features designed to increase accuracy over former work. The apparatus is schematically represented in Figure 1. *A* is a 50-watt Mazda lamp operating on a 110-volt A.C. circuit. It is enclosed in a light-tight

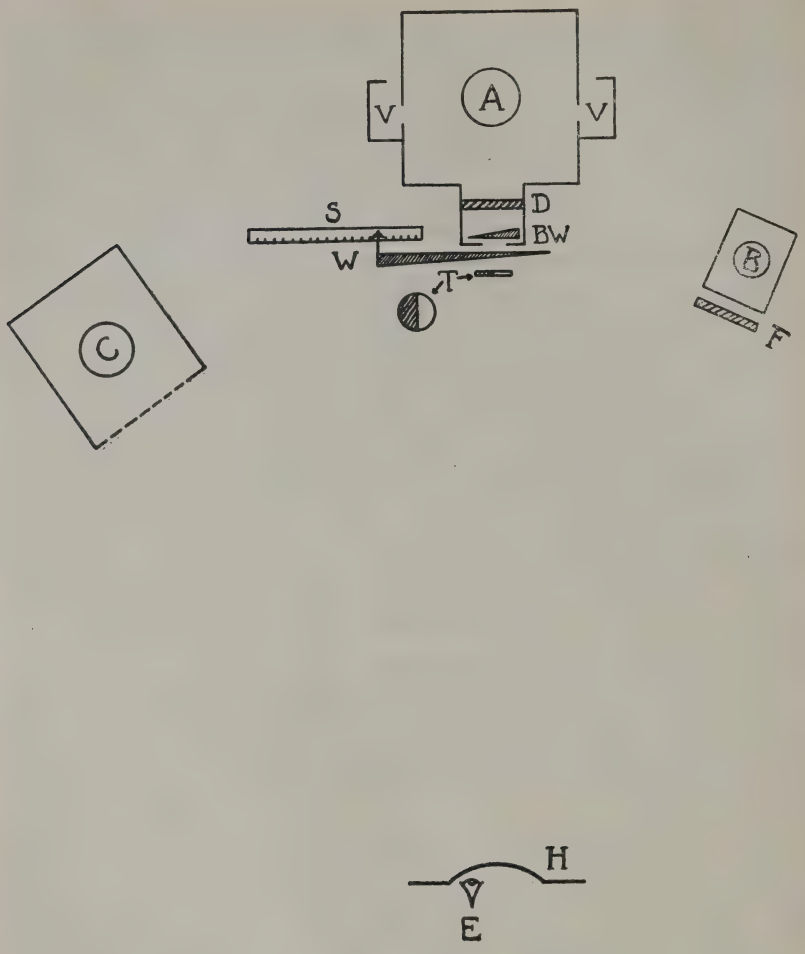

FIGURE 1

box, the interior and exterior of which are painted a dead black to minimize reflection. The efficiency of the lamp remains as nearly as possible at a constant level through the aid of the ventilating arrangement, *V*. Light from *A* is converted into "sensation daylight" by the filter, *D*, a No. 78 Wratten (Eastman), having a total transmission of 14%. The light thus reduced and changed in quality next passes through a balancing wedge, *BW*, and a neutral tint wedge,

W. The transmission of the latter is approximately 10,000 times as great at the clear end as at the other. It is mounted on a track in front of the filter box and may be moved transversely to alter brightness. This may be done either by the experimenter, as was nearly always the case in the experiments to be described, or by the subject. In the latter circumstance movement is effected by the turning of a geared wheel mounted near the subject, the motion being communicated to the wedge carriage by means of a flexible cable and rack and pinion. An indicator attached to the wedge passes over a calibrated scale, *S*. The scale readings may be translated into brightness units.

The light, thus further reduced in intensity, passes through a ground glass test object, *T*, before being transmitted to the eye of the subject, *E*. The visible portion of the test object has a diameter of 2 cm. and subtends a visual angle of approximately 31′. It is differentially ground to give a divided field; the transmission value of the dark side is 58.8% that of the light side. *T* is mounted on a circular rack and pinion; the position of the central line of division is capable of variation. The test object thus provides a fixed contrast surface which may be rotated to change the positions of the contrasting areas. In this experiment eight positions were selected; the dark side could be at the top or bottom, on the right or on the left, or in one of the following positions: upper right, lower right, upper left, or lower left. The position of the dark side was changed on each trial and the new location was determined by chance, the setting being made by the experimenter in the dark. For convenience the luminous surface of *T* will hereafter be referred to as Light *A*.

A second light, *B*, is located on the right of *A*, and the angle *AEB* is 5 degrees. Light *B* is a Spencer microscope lamp employing a 6-volt Mazda lamp; it is operated from a small transformer. Its luminous surface has a diameter of 29 mm. Interposed between *B* and *E* is a neutral tint filter, *F*, which serves to diminish the brightness of *B*. The latter has an intrinsic brightness of 1531 millilamberts. The filters used to supply a wide range of brightnesses to *E* have transmission coefficients of .50, .10, .01, and .001. In some of the trials, as will be noted later, *B* was used at full brightness (with no filter) and in others *B* was absent.

A third light, *C*, serves to illuminate a blind-spot pattern made by perforating the front of its light box. The points of light are so situated as to fall just within the limits of the blind spot of the left

eye and when invisible insure that the image of *T* will fall on the fovea.

The remainder of the apparatus consists of a head rest, *H*, clamped to a table at which the subject is seated, and a faintly illuminated control panel, not diagrammed, situated to the right and rear of the subject.

Briefly, the procedure was as follows: After an initial 4½-minute period of dark-adaptation [sufficiently long according to Hecht's measures (6) to insure complete foveal dark-adaptation], the simple contrast threshold was determined by the method of limits. All measures were taken upon *A*, the task for the subject being that of correctly designating the position of the dark side of the contrast. Between successive trials sufficient time was allowed for recovery from the slight fatigue effects of a single exposure. After an initial series of trials in which the threshold for the single light spot was obtained, other series were presented in which Light *B* was introduced and the thresholds were again determined. Table 1 serves to define the various conditions and to express results in terms of brightness units (millilamberts). The threshold values are the averages of the readings for four subjects. In no case do the values for one set of conditions overlap the range of those for another situation. This means that, statistically, the differences are completely reliable throughout. The same data are presented graphically in Figure 2, the thresholds in millilamberts being plotted against the intensities of the secondary stimulus, *B*.

TABLE 1

Conditions	Threshold*
Stimulus B absent	.0094 ml.
Stimulus B= 1.53 ml.	.0096 ml.
Stimulus B= 15.30 ml.	.0154 ml.
Stimulus B= 153.10 ml.	.0238 ml.
Stimulus B= 765.50 ml.	.0293 ml.
Stimulus B=1531.00 ml.	.0339 ml.

*Mean brightness of the two halves of the contrasting surfaces.

In this form our data are directly comparable to those of Spencer and, except for the measure of sensitivity (the *RL* rather than the *DL*), may be compared with Anderson's data. They merely indicate on the surface what appears to be a minor difference, traceable

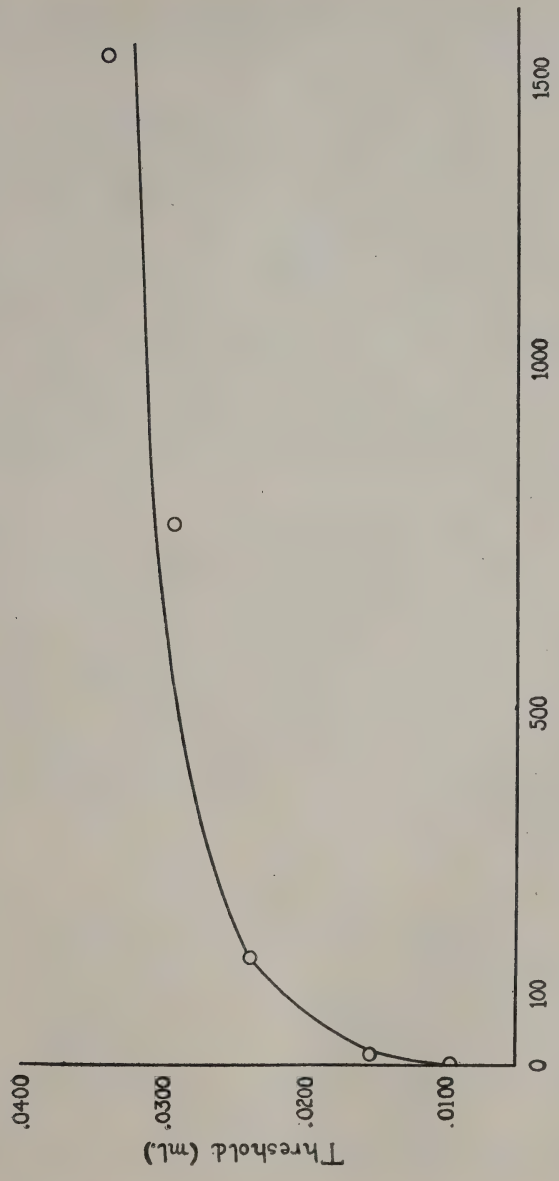
.0400
.0300
.0200
.0100
Threshold (mL.)
0
100
500
1000
1500
Brightness of 'B' (mL.)

FIGURE 2

presumably to errors of measurement in one set or the other. All data in this form combine to show that the effect of introducing a second light stimulus is to raise the threshold, i.e., lower the sensitivity in another part of the retina.

But the task of interpreting these results has only begun. An extremely important mechanism, the operation of which is present in all uncontrolled visual observation, needs to be reckoned with. Reference is made to the functioning of the pupillary reflex. It is obvious that, concurrent with the stimulation of the retina, there occurs an immediate and automatic adjustment of the size of the pupil to meet the new light conditions. With the increase in intensity of the environmental stimulus the pupil contracts to reduce the proportionate amount of energy actually reaching the retina. Retinal brightness is properly expressed, then, in terms which contain a correction for the prevailing pupillary conditions. Such correction is possible, fortunately, without recourse to direct measurement, which would be all but impossible in such a situation as that with which we are dealing.

By the use of the data compiled by Reeves (12) it is possible to determine the most probable diameter of pupil aperture for any given intensity of external illumination. Accordingly, the pupil diameters for our various conditions were determined, and from these values the pupil areas were computed. A simple mathematical operation, viz., obtaining the products of the thresholds (expressed in millilamberts) by the pupil areas (expressed in sq. mm.) yields values in terms of photons.[1] These measures express retinal illuminations in comparable units and may be regarded as correct stimulus limens. The tabulation of these figures discloses quite a different story (Table 2).

TABLE 2

Conditions	Threshold
Stimulus B absent	.3828 photons
Stimulus B= 32.82 photons	.2039 photons
Stimulus B= 173.40 photons	.1746 photons
Stimulus B= 865.00 photons	.1364 photons
Stimulus B=2912.00 photons	.1113 photons
Stimulus B=4350.00 photons	.0953 photons

[1]The "photon" has been variously defined (7, p. 261; 18, p. 27). Here the more convenient unit is employed.

These corrected thresholds, when plotted against intensities of Stimulus *B*, yield a curve which has an aspect quite different from that of our preliminary crude data. The curve is shown in Figure 3. It is but one step in the treatment of these data to determine the underlying relationship represented. Plotting the thresholds (in photons) against the logarithms of the intensities of *"B"* (in photons) a mathematical relation immediately becomes apparent (Figure 4). The equation that satisfies this curve has the form

$$RL = K \log I + C,$$

where, for these particular data, $K = -.051$, and $C = .284$.

Stated verbally, this means that the addition of a second visual stimulus serves to produce a positive facilitation, this facilitation being proportional to the logarithm of the intensity of the facilitating agent. Compare this with the statement of Heymans' Law according to which a positive inhibition should result under these conditions, the amount of inhibition being a function of the intensity of the "active" stimulus.

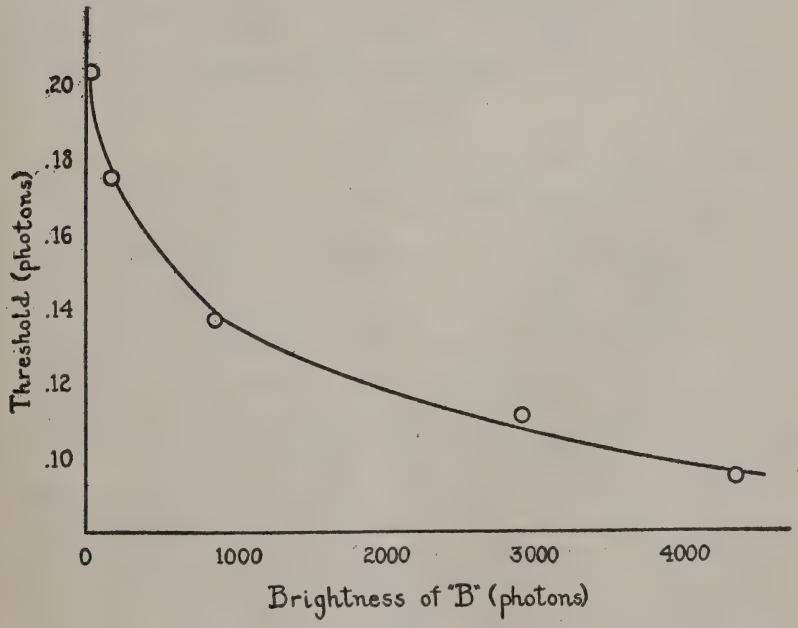

FIGURE 3

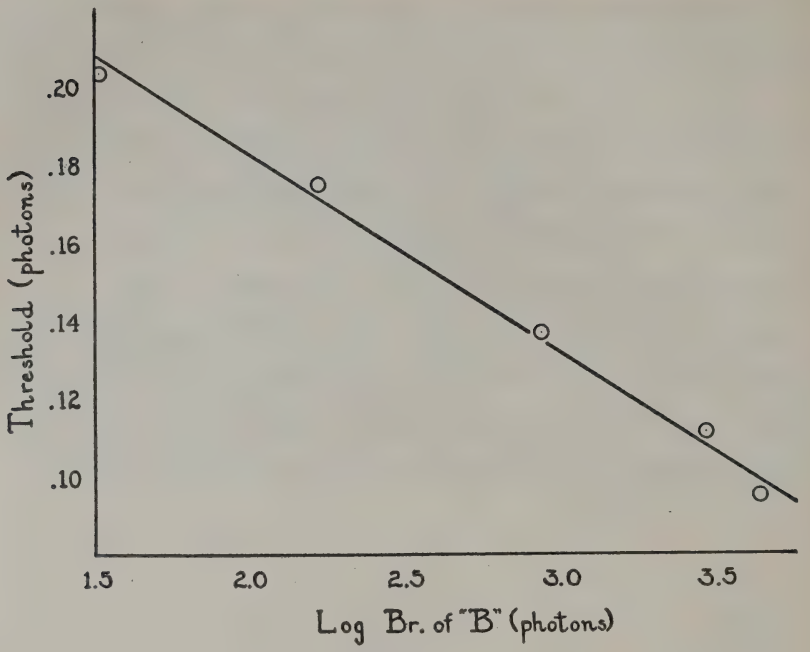

FIGURE 4

The writer has attempted to go back to the data of Spencer and Anderson and, by computation of their results, correct them to express what they properly should. But he has been hampered at every turn by the failure of both workers to state values in any absolute fashion. Light intensities, rather than finding expression in physical brightness units, are expressed entirely in relative terms. Relative values were, of course, the only requisites for these investigators, the matter of pupil contraction having been entirely disregarded by them. Unfortunately the average pupil has an absolute size for a given condition of external illumination and only with the knowledge of these measures could their data be reworked.

One other consideration should be mentioned in connection with the former experiments. It has been indicated that presumably critical experiments have been performed which proved conclusively the central nature of the alleged inhibition. Heymans believed it to be the case that the whole effect was one having its seat in the cerebrum and, as we have seen, Spencer performed an experiment which

once more demonstrated it. While we have no data directed upon this point, it may be confidently asserted that these experiments of earlier workers prove nothing of the sort. Spencer introduced his active stimulus in one eye, his passive in the other, and found the "inhibition" to be present as in the previous experiment. The results of the latter, being a function of pupil changes, point the way to a reinterpretation of the former. As is well known, the pupillary reflex is a consensual one; a change in the pupil of one eye is accompanied by a corresponding change in the other. This experiment, then, which should have been a critical one, merely demonstrates, as does the other, that the pupil contracts in response to light.

As a check upon the first series of experiments, which are needlessly indirect, a second series was performed with two subjects. Here the conditions were changed to render constant the rôle of the pupil. An artificial pupil, having a diameter of 1.4 mm. was inserted into the optical system. All other features of the experimental situation remained the same. The results are shown in Table 3. The absolute values in photons of both the thresholds and the brightnesses of Light *B* suffer alteration as a result of the diminution in pupil size; as retinal stimuli neither are as bright as in the former experiments. However, we are interested in the relation between the two variables.

TABLE 3

Conditions	Threshold
Stimulus B absent	.2154 photons
Stimulus B= 2.36 photons	.1842 photons
Stimulus B= 23.56 photons	.1560 photons
Stimulus B= 235.60 photons	.1324 photons
Stimulus B=1178.00 photons	.1266 photons
Stimulus B=2356.00 photons	.1121 photons

Figure 5 presents these results in graphical form. Here again a straight line relation maintains when thresholds are plotted against logarithms of the brightnesses of the secondary stimuli. The same general formula ($RL=K \log I+C$) is an expression of the curve. The slope and additive constants are somewhat changed for these data; here $K=-.022$, while $C=.180$. However, closer agreement with the values derived by the indirect method of computation would be remarkable indeed; we dealt there only with probable values of pupil area taken from Reeves' curves. The second set of constants

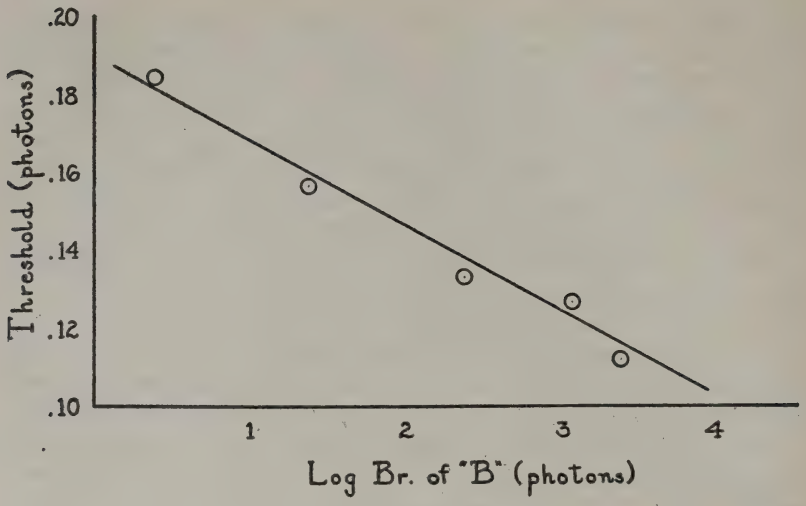

FIGURE 5

are doubtless more truly mathematically representative in spite of the smaller number of observations on which they are based. On the whole, the later experiment has proven a suitable check upon the first series and lends added weight to both the mathematical and verbal formulations of the principle that a positive facilitation is brought about in one retinal area as a result of simultaneous stimulation in another part of the retina.

It must be noted that the quantitative statement of brightness contrast here presented applies, in all probability, only to a situation employing the particular spatial relations actually represented in this work. There is no guarantee that an area further or less removed from the secondary or facilitating light stimulus would become sensitized to the same degree that the foveal region was in these experiments. An important question to raise is that concerning the retinal distance over which the transfer effect may be expected to operate. It appears certain that, for the intensities of secondary stimuli used in this work, there is no measurable increase in sensitivity at the fovea when the facilitating agent is situated as much as 15° away from the fovea. A short series of readings taken by the writer displayed no statistically reliable differences between threshold levels under conditions of no peripheral stimulation and a situation in which the secondary stimulus, situated 15° to the right of the fovea, had a brightness of 2356 photons.

On the contrary, what appeared to be an inhibitory effect was observed when Light *B* was moved into a position contiguous with Light *A*. It was at first thought that the normal facilitation process operating in the earlier experiments had been supplanted by an inhibition as a result of the difference in spatial separation of stimuli. However, partly due to the characteristic of all imperfect light sources that they scatter light and partly to the refraction of the cornea, lens, and other ocular media, Light *B* has a readily observed "halo" which, when the two lights are contiguous, overlaps the retinal test area and through fatigue substantially lowers the sensitivity of that region. This effect of diminishing sensitivity is appreciably reduced when lower brightnesses of *B* are used. This merely means that the actual physical scattering of light has been decreased with consequent reduction of fatigue in the test area. Fatigue has been shown to be related to intensity in this way (4, 5). There are doubtless two antagonistic factors in operation under these conditions, the induced heightening of sensitivity at the fovea, which it has been the chief purpose of this work to confirm, and the more potent fatigue from scattered light.

This suggests an explanation of the findings of certain investigators, notably Cobb and Geissler (2), who obtained a maximum enhancement of the contrast effect when they employed a surround which, far from being as bright as possible, was nearly equal in intensity to the test field. It is very probable that an increase in brightness of the peripheral stimulus would have introduced a fatigue in the contiguous foveal test area that would result in an overpowering of the increased facilitation attending more intense stimulation. It is certainly not the case, as we have already seen, that a maximum facilitation ensues from the employment of equivalent brightnesses in the facilitating and test areas. The lowest brightness of Light *B*, which produced the least facilitation, was more than ten times as great as the brightness of the test field.

The description of these two additional experiments, not entirely conclusive in themselves, serves to indicate the necessity for thorough and systematically planned experiments by which would be determined the spatial relations involved in brightness contrast. The suspicion is strong that the results of such research would display a progressive decrement in the slope constant of our logarithmic formula with the increase in distance between stimuli. Such a result is conjectural, however, and must await further experimentation.

Summary

Brightness contrast is given its correct expression in terms of the reciprocal relations maintained when two local retinal areas are simultaneously stimulated. Stating the contrast problem in these terms, a disparity is found to exist between two groups of experimental findings. The claim that stimulation of a peripheral portion of the retina sensitizes the fovea is controverted by Heymans' Law, which states that the reverse condition obtains. An experiment is described in which simple contrast thresholds were determined without peripheral stimulation and again under conditions of varying intensive degrees of such stimulation. Crude data derived from this experiment show a relatively insignificant deviation from the results to be expected if Heymans' Law were applicable. However, correction for pupillary changes reverses the trend of measures. A quantitative statement of brightness contrast is found to be given by the formula $RL=K \log I+C$, and the values fitting these data are ascertained. Heymans' Law is evaluated in terms of the present results and is found to be an artifact of faulty treatment of experimental findings. Particularly is the claim repudiated that former experiments substantiating the law have demonstrated the presence of "central inhibition." A second, more direct attack is described in which similar thresholds were obtained under conditions of pupillary control. The results of this experiment confirm the relationship found in the first work. A caution is issued against the generalization of the statement arrived at, two further situations being described in which it is shown that the facilitation effect breaks down at certain limits. In conclusion it is indicated that a systematic exploration of the retina would be necessary to establish the spatial relations involved in brightness contrast.

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LE CONTRASTE DE L'ÉCLAIREMENT ET LA LOI DE HEYMANS

(Résumé)

Le contraste de l'éclairement est exprimé correctement en termes des relations réciproques maintenues quand deux aires rétiniennes locales sont simultanément stimulées. Quand on exprime le problème du contraste en ces termes, on trouve, une disparité entre deux groupes de résultats expérimentaux. L'assertion que la stimulation d'une portion périphérique de la rétine sensibilise la fovea est opposée par la loi de Heymans laquelle nous dit que la condition se trouve la contraire. On décrit une expérience où, l'on déterminé des seuils simples de contraste sans stimulation pèriphèrique et encore dans des conditions de degrés intensifs de variation de cette stimulation. Des données pas très exactes dérivées de cette expérience montrent une déviation relativement insignifiante des résultats attendus si l'on pouvait appliquer la loi de Heymans. Cependant, la correction pour les changements pupillaires renverse la tendance des measures. Une assertion quantitative du contraste de l'éclairement se trouve dans la formule: $RL=K \log I + C$, et on trouve les valeurs qui conviennent à ces

données. On évalue la loi de Heymans en termes de ces résultats et on la trouve un résultat artificiel provenant d'un traitement faux des résultats expérimentaux. On nie surtout que les expériences antérieures pour prouver la loi aient montré la présence de "l'inhibition centrale." On décrit une seconde attaque plus directe où l'on a obtenu des seuils semblables dans des conditions du contrôle pupillaire. Les résultats de cette expérience confirment la relation trouvée dans le premier travail. On nous met sur nos gardes contre la généralisation des résultats, puis qu'on décrit deux autres situations où on montre que l'effet de facilité perd sa force à certaines limites. En conclusion, on indique qu'il faudrait une exploration systématique de la rétine pour établier les relations spatiales trouvées dans le contraste de l'éclairement.

GELDARD

HELLIGKEITSKONTRAST UND DIE HEYMANSSCHE REGEL

(Referat)

Der Helligkeitskontrast findet hier seine richtige Ausdrucksweise mittels der reziproken Verhältnisse die unterhalten werden, wenn zwei kleine Netzhautflächen zu gleicher Zeit gereizt werden. Wird das Kontrastproblem auf diese Weise ausgedrückt, so ergibt sich Mangel an Übereinstimmung zwischen zwei Gruppen von Versuchsergebnissen. Die Behauptung, die Fovea wäre durch Reizung eines peripherischen Teiles der Netzhaut empfindlich gemacht, wird bestritten durch die Heymanssche Regel, welche behauptet, die Lage sei eben die umgekehrte. Es wird ein Versuch beschrieben, worin einfache Kontrastschwellen (contrast thresholds) ohne peripherische Reizung und wieder bei wechselnder Stärke solcher Reizung festgestellt wurden. Unbearbeitete Ergebnisse, aus diesem Versuch gewonnen, offenbaren eine relativ unbedeutende Abweichung von den Resultaten, die im Falle der Anwendbarkeit der Hermanschen Regel zu erwarten wären. Veränderungen der Pupille stellen aber die Richtung der Abmessungen um. Es findet sich, dass eine quantitative Darlegung des Helligkeitskontrastes in der Formel $RL = K \log I + C$ gegeben ist und die für diese Ergebnisse passenden Werte werden festgestellt. Die Haymanssche Regel wird in Bezug auf die gegenwärtigen Ergebnisse abgeschätzt und offenbart sich als Kunstprodukt durch fehlerhafte Behandlung der Versuchsergebnisse bedingt. Man bestreitet besonders die Behauptung, frühere, die Regel bestätigende Versuche hätten das Bestehen der zentralen Hemmung (central inhibition) bewiesen. Ein zweiter, mehr direkter Angriff wird geschildert, worin ähnliche Schwellen festgestellt wurden, bei Kontrolle der Pupille. Die Ergebnisse aus diesem Versuche bestätigen das in der ersten Arbeit gefundene Verhältnis. Man mahnt gegen Veralgemeinerung der erzielten Behauptung, und es werden zwei weitere Situationen beschrieben worin gezeigt wird, dass die fördernde Wirkung an gewissen Grenzen scheitert. Zum Schlusse wird angedeutet, das eine systematische Untersuchung der Netzhaut nötig sine würde, um die Raumverhältnisse bei Helligkeitskontrast festzustellen.

GELDARD

LEARNING AS A PROPERTY OF PHYSICAL SYSTEMS*

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N. Rashevsky

Of all the phenomena exhibited by living organisms, those of learning, and more generally all phenomena associated with the functioning of the central nervous system of higher animals, seem to present the greatest difficulties to an interpretation in terms of known physical laws. No valid proof of the impossibility of such an interpretation has ever been given, and yet no physical explanation of the general properties of learning based on one or a few simple physical principles is known. Perhaps the reason lies not so much in the lack of attempts by biologists, as in the lack of interest shown by physicists in this field. Whenever a biologist states that such or such a phenomenon is not known in physics, he bases his statement on what the physicist tells him. And if the study of a certain class of physical phenomena, which are of importance for biology, is for some reason negelected by physicists, it is no wonder that the biologist has made little headway in physically explaining certain properties of the living.

The purpose of this paper is to fill the gap existing in physics in regard to systems which exhibit phenomena, that the biologist would call learning, if he found them in living organisms. It must be strongly emphasized that it is not our purpose to give a physical theory of the functioning of the central nervous system of higher animals. In our opinion, such an attempt would be premature, for the reason that in order to establish a physical theory for a particular system, we must possess a general theory of systems which exhibit the properties in question. The development of celestial mechanics, which includes the study of the motion of bodies which attract each other according to Newton's law of force, varying as the inverse square of the distance, had to be preceded by the development of general mechanics, which studies more generally the motions under

*Recommended by K. S. Lashley, accepted for publication by Carl Murchison of the Editorial Board, and received in the Editorial Office, April 21, 1930.

the influence of *any kinds* of forces. Similarly here: before attempting to establish a physical theory of the brain, we must make a systematic theoretical study of such systems as possess the *general properties* of the central nervous system. Only after the physicist has shown what kind of systems of that type are physically conceivable, will the physiologist be able to decide which types of these physically conceivable systems are actually exemplified by the brains of higher animals. We do not propose to introduce new hypotheses, *ad hoc,* for the explanation of learning; our problem is stated thus: given the well-established kinetic and thermodynamical principles of physics, can we deduce by purely logical methods from these principles the possibility of existence of such physical systems as exhibit the fundamental properties of learning?

If considered from a purely physical point of view, the most striking characteristic of the central nervous system is this, that under apparently quite similar external conditions, its reaction to a given external change may be quite different, depending largely on the previous history of the system. Some biologists, e.g., L. v. Bertalanffy (2) declared that this historical character is quite foreign to physics and that all physical equations are of such a kind that the state of a system described by them is determined solely by the present conditions of the environment of the system, regardless of the previous history. That this is not true and that generally the physical equations comprise also the past history of a physical system, has been recently shown by the author (19). Even apart from such general statements, we know phenomena in physics which exhibit an historical character: the so-called hysteresis. An originally non-magnetized iron bar, when subjected to a gradually increasing external magnetic field becomes more and more strongly magnetized. When the external field is again reduced to zero, in other words, when the original external environment is re-established, the bar remains somewhat magnetized. When again subjected to a magnetic field, which increases in the same manner as before, the "reaction" of the bar is a different one (for instance, the slope of the new magnetization curve is gentler). The amount of this residual magnetization, and hence also the "reaction" of the bar to a given variation of the external magnetic field, *depends largely on the way in which the magnetic field was varied the first time.* Similar phenomena of hysteresis are known in plasticity and in the behavior of colloids (1). All the different theoretical explanations made for the phenomena of

magnetic or plastic hysteresis finally reduce to the assumption that the phenomena in question are due to the existence of several positions of equilibria for the atoms (or electrons in the atoms) in a given external field of force (3, 6, 12). A simple mechanical illustration may be obtained by considering a ball rolling on a curved surface which has several depressions. The ball will finally fall into one of the depressions and remain there permanently, unless some outside force lifts it out. In such a depression the ball is in a stable equilibrium in this sense, that after small displacements it will return to the lowest point of the depression. At this lowest point, its potential energy has a minimum. But since we assumed several depressions on the surface, the final position of the ball is not determined completely by the shape of the surface. The ball may remain permanently in any of the depressions. In other words, the state of the system is not completely defined by the environment, and may be different for equal environments. But it depends largely on the previous history of the system. If, for example, the ball should be thrown on the surface, the depression in which the ball will finally remain at rest will be determined by the initial velocity and direction.

The equilibrium of any non-mechanical, physico-chemical system is determined in a way which formally resembles the case of the ball on the curved surface. For phenomena occurring at constant temperature, any such system tends to acquire such a state, for which a certain quantity, called the free energy, has the smallest possible value. The "state" of the system is hereby specified by different variables, such as amounts of different substances present in the system, concentrations, etc. The free energy is a function of these variables, which we shall call the thermodynamical variables; for quite definite values of the thermodynamical variables the free energy will have a minimum value and the system will be in equilibrium for these values. To simplify matters, let us consider the case of a system whose state is determined by one variable only. Such would be, for instance, the case of a solution of a substance some of whose molecules are dissociated. The ratio of the dissociated molecules to the total number of molecules characterizes such a system completely. To every value of this ratio there corresponds a certain amount of free energy. If the free energy is plotted against this "dissociation ratio," a curve like that shown in Figure 1 is obtained. The system has this important property however: that the shape of the curve depends also on a number of other variables which specify the environment of

the system. One such variable is the temperature. If we plot the free energy F against the dissociation ration x for different temperatures T' and T'', we obtain slightly different curves.

Considering now the system, when it is kept at the temperature T', we see that for a definite value x' of the dissociation ratio the free energy has a minimum. Therefore at this temperature the system will be in a stable equilibrium for a dissociation ratio x' and will remain in this state as long as the temperature is kept constant. If, however, we change the temperature to T'', we see that for this temperature the minimum of the free energy will correspond to a value x'' of the dissociation ratio. Hence, the change of temperature will result in a change of the dissociation ratio. But as long as the F, x curve has only *one* minimum, for every given temperature there corresponds *one* quite definite state of equilibrium of the system. In other words, the state of the system is in this case completely determined by the environment. This holds for systems with any number of variables, as long as the free energy has only one minimum.

Practically in all cases the systems hitherto considered in thermodynamics are of such a kind that the free energy has only one minimum. Nothing, however, stands in the way of investigating the general case, in which the free energy may have any number of minima. Such cases eventually have been encountered by students of thermodynamics (10), but no great attention has been paid to them because of their extreme exceptionality in practice (case of undercooled vapors or liquids).[1] But with the purpose which we have in view, just this exceptionality makes a systematical investigation of these cases very interesting. We may hope to find here similar cases of hysteresis, as in the case of the ball, and thus approach the problem stated at the beginning.

The investigation may follow two different directions. First, we can make a purely thermodynamic study of the properties of systems which have several equilibria, without making any special assumptions about their physical constitution. The fascination of thermodynamics

[1]If water is cooled very rapidly, it may remain liquid at several degrees below freezing point. That means that within a certain range of temperature it possesses two states of equilibria, a solid and a liquid, so that the environment alone, in this case, temperature, does not specify definitely the state of the system. However, the equilibria corresponding to the liquid state is less stable in this particular region of temperatures. A sufficiently strong concussion or any other disturbance will cause the water to freeze. Thermodynamically, however, this limitation is not essential and we may conceive systems with two equilibria which are equally stable.

lies especially in that it allows us to obtain many important propositions about physical systems without making any special assumptions. We shall see in the following that from purely thermodynamical considerations, based on a geometrical consideration of curves with several minima, we may deduce many important properties exhibited in learning. The second way is to interpret the thermodynamical results thus obtained from a molecular-kinetic point of view. Certain progress has recently been made in both these directions by the author in a series of publications which shall be referred to subsequently. As regards the molecular interpretation of systems with several equilibria, it has been shown (21) that no special assumption about the forces between atoms and molecules are necessary and that under somewhat special conditions, forces of attraction and repulsion, which are quite familiar to the physicist, and which are responsible for a great number of known physical phenomena, are sufficient to account for the existence of more than one minimum of free energy of a system.

Now let us follow the purely thermodynamical way. We shall hereafter restrict ourselves to systems with only one variable, like the system considered in Figure 1. For the sake of generality, however, we shall not interpret this variable necessarily as a dissociation ratio. Its physical meaning may be quite different, and this does not concern us here. We shall denote the thermodynamical variable by λ. Similarly, we shall consider the general case that the shape of the curve which gives the relation between F and λ depends on different environmental conditions. We considered as such the temperature in Figure 1. But besides the temperature, there may be a number of other physical quantities which specify the environment, such as pressure, electric charges, or the presence of some other "foreign" substances. Thus the addition of a third substance to the solution in the case of Figure 1 may also change the curve for the free energy, or in other words, it may influence the dissociation ratio. The addition or removal of such a substance from without has also to be considered as a variation of the environment in the most general sense. These physical quantities, which specify the outside environment, we shall call *external parameters,* and denote them with α, β, γ, etc.

After these preliminaries we may pass to the consideration of our general case, where the curve which represents the variation of the free energy F with λ has several minima (Figure 2). The shape of this curve, as we have seen, depends in general on the values of

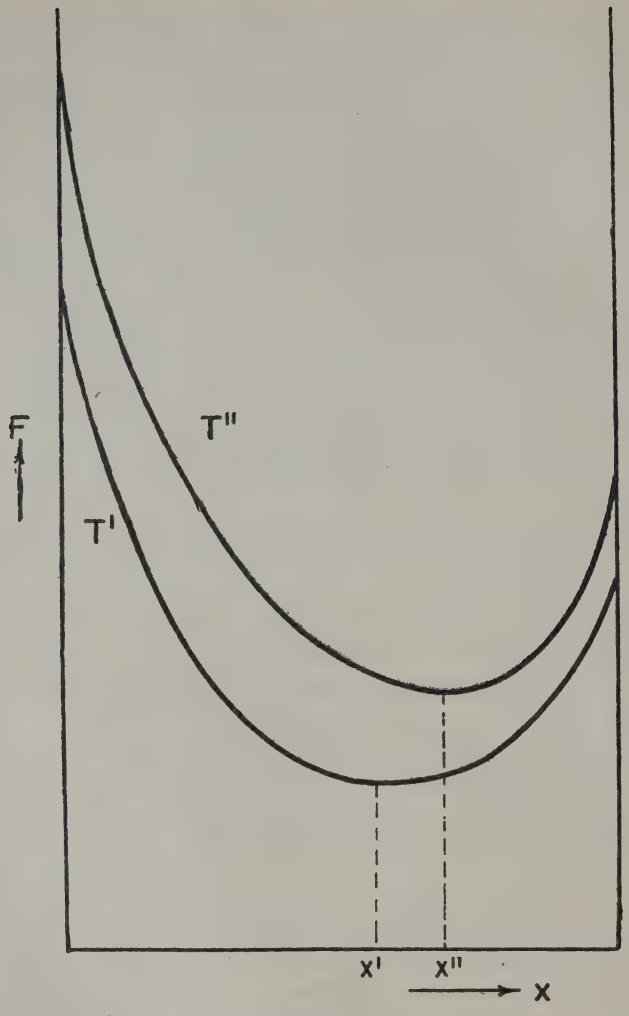

FIGURE 1

a, β, γ, . . . Let us consider the variation of this shape as depending on the variation of one of the parameters, say a, the others being kept constant. In Figure 2, the curves are drawn for various values of a: $a^{-'}$, a°, a', a'', a''', etc. With the variation of the shape of the F, λ curve, not only the positions of the minima will be generally

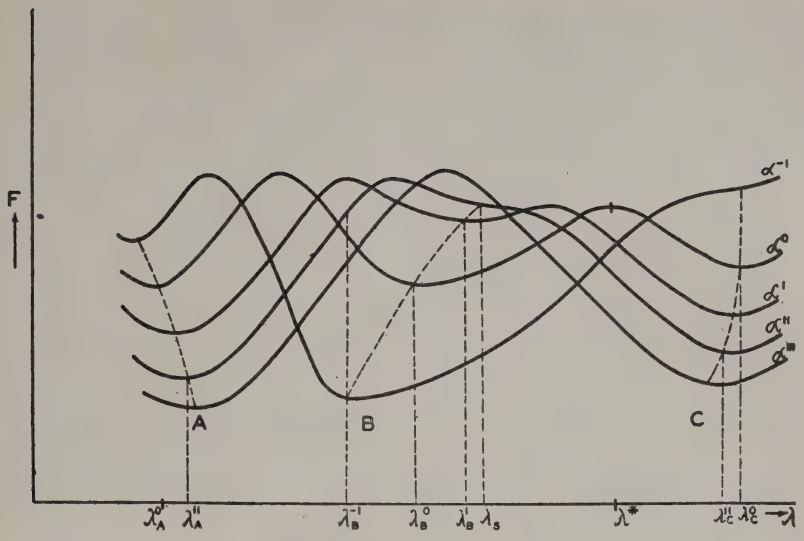

FIGURE 2

displaced but some of the minima may disappear, "flatten out," as is represented on Figure 2 for $a = a''$ for the minimum B.

Let us start our considerations with such an environment, which corresponds to a value a° of a. Since for this value of a the F, λ curve has three minima, A, B, and C, the equilibrium state of the system is not determined completely by the environment like the several equilibrium states of the ball on a surface with several depressions. For a given environment, specified by $a = a^\circ$, the system will be in equilibrium for any of the three values λ_A°, λ_B°, λ_C° of the thermodynamical variable. Suppose that it is in a state determined by λ_B°, that is, corresponding to the minimum B. Let us now vary a from a° to a'''. We see that the position of the minimum B shifts towards greater λ. That means if we vary a from a° to a' the value of λ which corresponds to the equilibrium, will change from λ_B° to λ_B'. But as long as the value of a'' is not reached, that is, as long as the minimum B does not disappear, the corresponding equilibria will be stable. For any small change of λ in either direction will correspond to an increase of the free energy, and λ will vary in such a way that the free energy should decrease; that is, it will turn back to the value corresponding to the minimum of F. A further important

thing is this, that as long as λ did not reach the value λ for which the minimum B disappears, the system will return to its original state whenever the original environment $a = a^{\circ}$ is restored. But consider now the case that a varies up to a''. At the moment a'' is reached, the state of the system will correspond to λ_S. But this will not be a state of stable equilibrium, for it does not correspond to a minimum of the free energy. λ should now vary in such a way, that F decreases; that is, λ will increase until it reaches the value λ_C'' corresponding to the "nearest" minimum C. If now we again restore the original environment, by making $a = a^{\circ}$, the system will not return to its original state but to the state of $\lambda_C{}^{\circ}$, which is also a state of equilibrium and in which it will remain as long as a is kept constant. In order to bring the system back into the state $\lambda_B{}^{\circ}$ we must now vary a in the opposite direction, until it reaches the value a'. For this value of a the minimum C disappears, and the system returns to $\lambda_B{}^{\circ}$.[3] We see that in order to know the state of the system for a given environment, the knowledge of this environment is not sufficient. We also must know how this environment varied in the past (16).

An immediate consequence of the foregoing discussion is that, in order to bring the system from one possible state of equilibrium into another, the variation of the external parameter must exceed a certain threshold value, a'' in case of Figure 2. But a still more important property of such systems is this, that even if the threshold value is exceeded, the environment change must last for a sufficiently long time in order to bring the system from one state of equilibrium into another. This is due to the fact that, whenever any physico-chemical system is not in equilibrium, it always takes a finite time to reach the state of equilibrium (16). Now suppose a acquires the value a'', the system being originally in the state $\lambda_B{}^{\circ}$. λ will start to

[3]It has been suggested by Professor K. Swartzel that the whole phenomenon may be easily visualized by imagining a bead on a wire. The wire can be given the shape of the various curves of Figure 2. The deformation of the wire corresponds to the variation of the environment. In any case, the bead will tend to assume the lowest position on the wire. If we originally place the bead in the position B, and then deform the wire so as to make it correspond to a°, a', a'', a''', etc., and then bring it back to the original shape a°, the correctness of the foregoing somewhat abstract argument can be seen at once. The bead must move on the wire with sufficient friction, so that when displaced from a minimum, it should reach the minimum *aperiodically* and not oscillate around the position of equilibrium. Oscillating processes in thermodynamics are possible, but very exceptional.

vary from λ_S to λ_C'', but this variation requires a finite time. If now we restore the original environment a° before λ has reached the value λ^*, we see that λ will again begin to decrease towards λ_B° for the minimum B is "nearer" than C. Hence, in spite of having reached and eventually exceeded the threshold value a'', we cannot bring the system into the state C, unless we keep a'' for the time necessary for λ to exceed λ^* (20).

It can be seen, however, that the system can be brought into another state of equilibrium even when the duration of the varied environment is very short. But, in this case, these impulsive variations must be repeated sufficiently fast and a sufficient number of times (20). This is again due to the finite velocity of variation of λ. If a is brought back from a'' to a° before λ reached λ^*, λ will tend back to λ_B°, but again it will require a finite time before λ reaches λ_B°. If, before this happens, a is again changed for a short time to a'', λ will start towards λ^* from a point where it is nearer to λ^* than at the beginning of the first impulse. It will therefore move during the second impulse somewhat further than during the first, and so on. After a sufficient number of impulses λ will finally exceed λ^*, and the system will pass to λ_C'' even upon returning to the original environment. The problem can be treated mathematically, and the necessary number of repetition calculated, if the shape of the F-curve is given (20), for as a first approximation the rate of change may be taken to be proportional to the steepness of the F, λ curve (18).

Another very important property of systems with several equilibria is that their state is determined not only by the past variations of the environment, but also by the *rate of these past variations* (16). If we start with a system in the state B for a^- and vary a to a''' slowly, everything will happen as described before; that is, the system will finally pass into the state C. But if, starting with a^- and λ_B^- again, we vary a from a^- to a'' *very suddenly,* so that the rate of variation of a will be much larger than the rate of variation of λ, then the system will pass into the state A. For while the F, λ curve will already have the shape corresponding to $a = a''$, λ will have practically still the same value λ_B^-. But a system in λ_B^- for the value $a = a''$ will tend to reach the minimum A of F.

In general, the environment of the system is described by several external parameters a, β, γ, . . . etc. Let us now consider the case of two external parameters, a and β, and in order not to lose our-

selves in generalities, let us consider one of the many possible cases as represented in Figure 3 (21). The F, λ curve varies differently with the variation of α and β. In Figure 3 the shape of the curve is

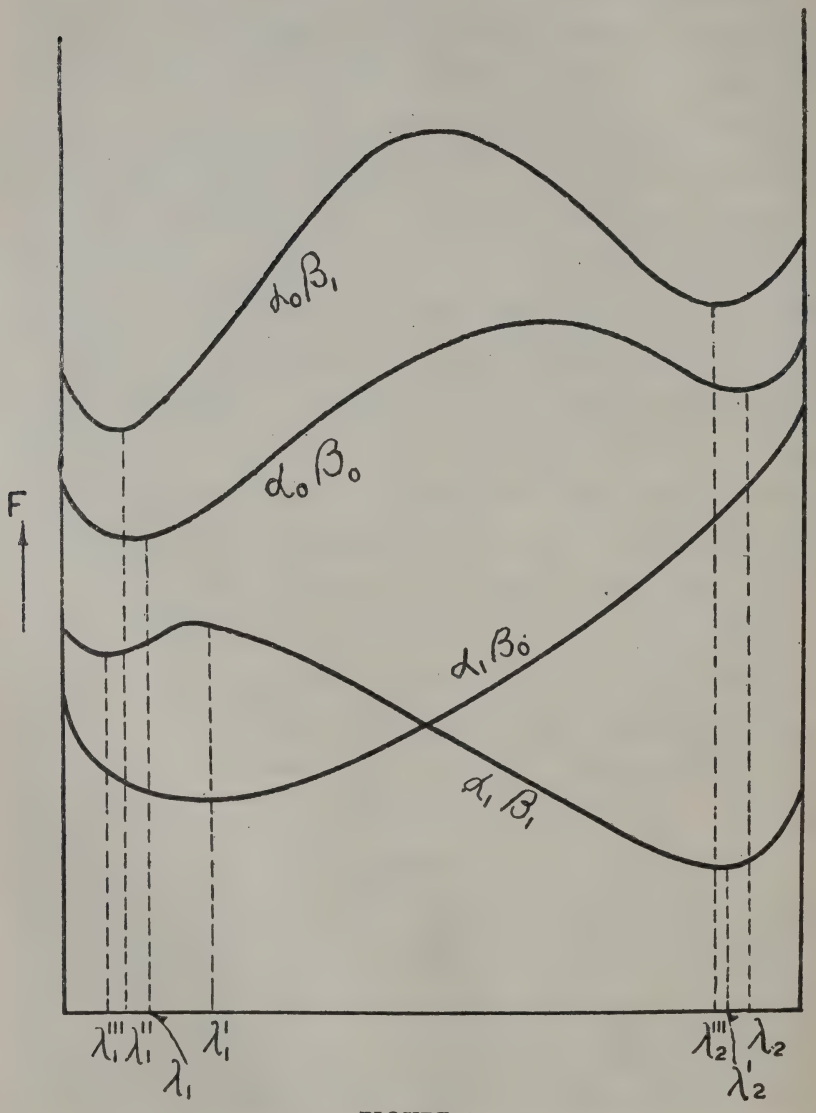

FIGURE 3

represented for four different combinations of the values of α, β: $\alpha_0 \beta_0$, $\alpha_1 \beta_0$, $\alpha_0 \beta_1$, $\alpha_1 \beta_1$. Let us start with environment, which corresponds to the values $\alpha_0 \beta_0$, and let us denote the state of the system corresponding to the first minimum, State I, and that corresponding to the second minimum, State II. Let the system originally be in State I. If we vary now α alone, we obtain the curve $\alpha_1 \beta_0$. The equilibrium value of λ is shifted from λ_1 to λ_1'; but upon return to $\alpha_0 \beta_0$ the system returns again into State I. A variation of β alone gives the curve $\alpha_0 \beta_1$. Again λ is slightly shifted, but again the shift is reversible. Let us now vary *first* α and *then,* retaining the new value of α, vary β. The variation of α brings the F-curve into the shape $\alpha_1 \beta_0$, and λ into λ_1'. The subsequent variation of β brings the curve F into the shape $\alpha_1 \beta_1$. Since λ_1' lies to the right of the maximum of $\alpha_1 \beta_1$, λ will tend towards λ_2', which corresponds to the second minimum of $\alpha_1 \beta_1$, and if sufficient time be allowed, or if the process be repeated several times, the system will pass into the State II upon restoration of the original environment $\alpha_0 \beta_0$. If now β be varied alone, λ will be shifted from λ_2 to λ_2''' in a reversible way. But if α is varied alone, so that we get the curve $\alpha_1 \beta_0$, and if this is repeated sufficiently often, λ will vary from λ_2 to λ_1', and upon restoring $\alpha_0 \beta_0$ again, the system will return into State I.

But if starting with State I, and external condition $\alpha_0 \beta_0$, we shall vary first β (giving $\alpha_0 \beta_1$) and next α (giving $\alpha_1 \beta_1$), the system will not pass into State II, however long or often we repeat the procedure. For if we vary *first* β, we obtain the curve $\alpha_0 \beta_1$, and λ is changed to λ_1''. If now we vary α so as to get $\alpha_1 \beta_1$, we find that since λ_1'' lies to the left of the maximum of $\alpha_1 \beta_1$, λ will tend towards λ_1''' and not towards λ_2', so that upon restoring the original environment $\alpha_0 \beta_0$ the system will return to λ_1.

Summarizing, we have the following scheme: if for a certain environment $\alpha_0 \beta_0$ the system is in the State I, then variations of α or of β alone cause only reversible changes in the system. But a variation of α followed by a variation of β brings the system into the State II. However, if the *order of application of the variations of* α *and* β *be reversed,* the variations will remain ineffective in bringing the system into the State II. If, when the system is in the State II, we vary α alone, without "reinforcing" it by a variation of β, the system returns into State I. This recalls, though in a very remote way, some effects of the conditioned and unconditioned stimuli. More interesting properties are obtained by further generalizing our con-

siderations. Hitherto, we have considered those systems which possess *one* definite threshold for the external parameters (a'' in Figure 2). But, in the general case, it may happen that when an external parameter varies continuously in one direction, a minimum on the F-curve will disappear for a certain value a_3 of the parameter, and reappear for a certain other value a_4 (Figure 4). In such a system a variation of an external parameter will be effective in bringing the system into a different equilibrium state only if this variation lies between certain limits, a_2 and a_4. It is true that, if we vary a above a_4, a will necessarily assume (for a while) values lying in the interval $a_2 - a_4$, but if the variation of a is sufficiently rapid, this interval will be passed in a short time, and according to

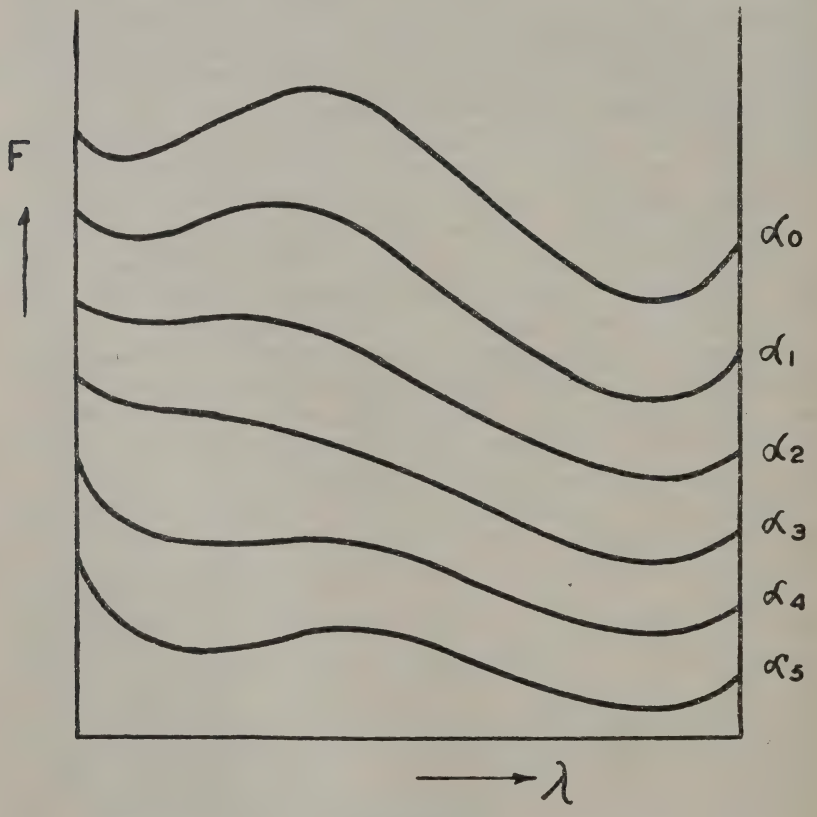

FIGURE 4

what has been said before, the system will not be able to pass from one possible state into another. If the interval $a_2 - a_4$ is very small, the transition of the system from one state into another will occur practically for only one definite value of the parameter, $\alpha = a_3$. More exactly for $\alpha = a_3 \pm \Delta$ where Δ is a very small quantity. Instead of a lower limit, or a threshold, we have now a "critical value" (a_3), which an external parameter must have, and neither exceed nor fall short of, in order that a certain irreversible change in the system should take place. A physical example of such a case may be found in the coagulation of certain colloids, which takes place for quite a definite concentration of the hydrogen ions, for which the charge of the particles becomes zero, and does not occur for smaller or larger concentrations for which the change is either positive or negative (4, 9).

Now let us consider a very large number of systems, which we shall call *s*-systems, of which every one possesses *two* minima for its free energy, and whose thermodynamical behavior is represented by the diagram of Figure 3, with the only difference that whereas for the parameter β there exists a threshold for the parameter α there should exist a "critical value:" All *s*-systems should be similar, except that different *s*-systems may have different "critical values" for α (19, 21). The whole collection of the *s*-systems we shall consider as one *S*-system. Example: a colloidal solution is a system composed of an enormous number of individual particles, each of which is a system in itself.) Such an *S*-system can be shown to have the property that, when subject to a variation of the environment, it behaves as follows: originally the system does not change its state when either α or β is varied alone. But if α is varied so as to assume a definite value a_i and this is followed by a variation of β, the system acquires, after sufficient repetition of the procedure, the property of changing its state when α is varied alone; but this change of state occurs only when α assumes *the value* a_i.

To give an illustration of these abstract considerations, let us consider a colloidal solution, composed of particles of different size, or more generally, of different kinds. Particles of different size will in general have also a different "critical value" for the pH at which their coagulation begins. Now consider the case in which the physico-chemical constitution of each particle is such that it possesses two states of equilibrium, so that its behavior is described by the diagram of Figure 3, except that for one of the parameters, α, there exists

not a threshold, but a "critical value." Let the physical meaning of α be the pH of the solution, and let the "critical value" of α coincide with the critical pH, for which the charge of the particle becomes zero. Let the physical meaning of β be the concentration of some other substance in the solution, which influences in a definite way the constitution of the particles. As in the case of Figure 3, we shall speak of "State I" and "State II" of a particle.

Let all the particles in State I be so stable that they do not coagulate even when their charge becomes zero. In State II, however, all particles of the kind i will be coagulated at a pH, which equals n_i. Now if hydrogen ions are added in a concentration n_i and the substance β is added subsequently, and this is repeated several times, all the particles of the kind i will pass into the State II, because, as assumed, n_i is also the "critical value" for the transition of a particle of kind i from I into II. If now hydrogen ions are added alone, they will not be effective, unless taken again exactly in the concentration n_i. For only the particles of the kind i are now in a State II, and they will be precipitated only by the concentration n_i. That is, we can make the colloid give a precipitate at a definitely chosen concentration n_i, if we subject the colloid to that concentration, and then follow this every time by the addition of a proper substance. From the circumstance that the behavior of each particle is governed by the scheme of Figure 3, it follows: first, that the addition of hydrogen ions *must always precede* that of β, and, secondly, that repeated addition of hydrogen ions alone, without reinforcement by addition of β, will make the colloid again insensitive to the hydrogen ions (21).

Although such phenomena are not yet known experimentally in physical chemistry, their possibility follows directly from the systematic consideration of complicated systems with several equilibria. It must also be emphasized that this case of a colloid showing the particular behavior is only an *illustration* of our *S*-system. This general property of the *S*-systems can be deduced from purely thermodynamical considerations about the interaction of a large number of systems, each having several equilibria. The meaning of α should not necessarily be the pH, and the response to a definite value of α may consist in any other physical change of the system.

The *s*-systems, considered above, may or may not be homogeneous (21). An *S*-system, as composed of many *s*-systems, is necessarily heterogeneous. Now let us make a further step and consider a still more general heterogeneous system, *L*-system, which will include as

its parts a number of *S*-systems, but some parts of which may be ordinary systems, with one equilibrium, of any constitution. The study of such extremely hetereogeneous systems is particularly justified in view of our problem, because living organisms exhibit the greatest heterogeneity. Even apart from our special problem, the study of such heterogeneous, cellular systems which possess a definite structure, throws interesting light on biological phenomena (8) and has formed the subject of several recent publications of the author (13, 14, 15, 17). A general property of any extremely heterogeneous system is that changes in the environment naturally do not influence all the parts of the system in the same way. There will be only certain parts which we shall denote as *a*-parts, and which will exhibit changes in their physico-chemical constitution under the direct influence of any changes of environment. In the general case, one definite *a*-part will "respond" by a variation of its constitutions to one definite change of the environment, another *a*-part to another change. The physico-chemical changes which occur in the *a*-parts may in their turn cause changes in other parts, *e*-parts, which, due to the particular structure of the systems, will not be influenced directly by changes in the environment. The physico-chemical changes in *a* will in general be connected with the formation of certain substances. For simplicity, let us assume one substance, *A*-substance. Similarly, an *E*-substance is formed when a change is brought up in an *e*-part. We thus have the scheme: change of environment causes a change in one or several *a*-parts, consisting in the evolution of *A*. This affects certain *e*-parts, and causes the production of *E*. No hypotheses are made about the nature of *A* and *E*.

Some *a*-parts may lie adjacent to *e*-parts in the system ($a_r e_r$ Figure 5). Between some other of these may be interposed *S*-parts, which are our *S*-systems, considered above. A change in a_r will cause a change in e_r, due to their adjacent position. As regards a change in a_i, it may, or may not cause a change in e_r according as the *S*-parts do or do not transmit the action from *a* to *e*, whatever the physical nature of this action may be. The cases that *S*-parts either always transmit the action or always stop it lead to trivial results. Of much greater interest, on the contrary, is the case that the *S*-systems normally do not transmit this action, but that they become transmitting as a result of response to a cerain concentration of *A*. In other words, when *A* plays the part of α, *E* that of β, and the response of the *S*-system (coagulation in our example with the colloid) consists in be-

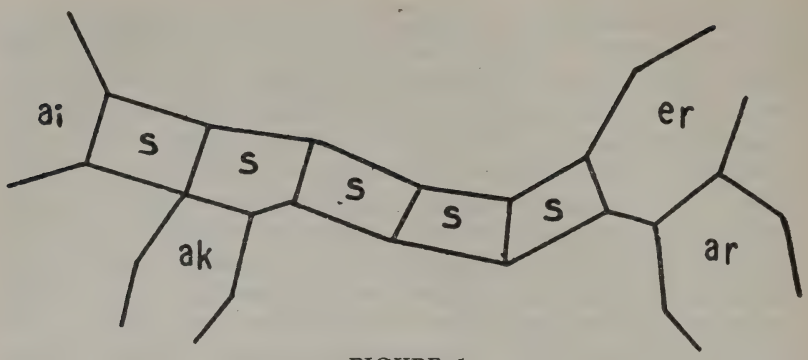

FIGURE 5

coming transmitting. In this case, by merely changing the notations of pages 219-220, we consider the following case: S is originally non-transmitting, and remains so when A or E act on it alone. But if a certain definite concentration of A, say c_i acts on S, and this is followed by the action of E, S becomes transmitting whenever A is applied alone in the concentration c_i. Often application of A alone renders S again non-transmitting.

A and E are assumed to diffuse through the whole system. We assume that the rate of diffusion of E is much greater than that of A, so that it reaches practically all parts of L at once. This assumption is made only to simplify the following considerations and is not absolutely necessary.

Whenever any part a_i is undergoing changes under the influence of the variation of the environment, we shall speak of the "influence a_i." If an influence a_i causes a change in the part e_r, we shall call this change in e_r the reaction e_r to the influence a_i, and shall furthermore say that the reaction is "unconditional" if it *always* follows the influence. This will happen when a_i and e_r are adjacent. We shall say that an influence a_i is "neutral" when it does not cause any reaction e.

When an a-part is influenced, a diffusion wave of A is propagated through L. The concentration of A at any time for any point is determined by the diffusion equation. The important point is this, that this diffusion field is different for different ways of influencing the a-parts. Schematically, the situation is shown in Figure 6. For a given moment, the concentrations of A are shown above the different S-parts for this case, when a_i is influenced alone (i); when first a_i,

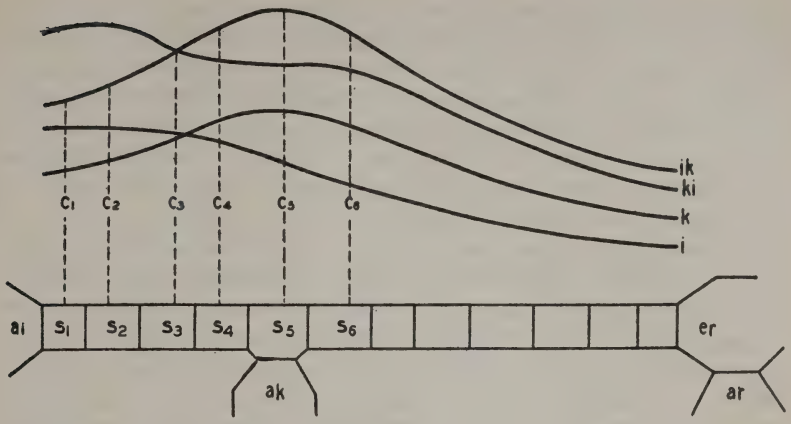

FIGURE 6

then a_k is influenced $(i\,k)$; and when first a_k, then a_i is influenced (ki). That is, different patterns of influences produce different diffusion fields of A. Now let us consider one definite pattern of influence $(a_i a_k)$. Neither the influencing of a_i or a_k alone can produce any change in e_r because the S-parts lying between are non-transmitting. But if, after having produced an influence pattern $a_i a_k$, we influence a_r after an interval of time t, the following happens. Let the concentration of A at the time t be given by the curve ik (Figure 6). At this moment a_r is influenced and causes a reaction in e_r, that is, E diffuses through the system L. Hence the part $—S_1$ is acted upon by a concentration c_1 of A, and by E; part $—S_2$ by a concentration c_2 of A and by E, etc. If the procedure be repeated, care being taken to influence a_r always t seconds after $(a_i a_k)$ we see that, according to what has been said above about the S-systems, S_1 will become transmitting when acted upon by A alone, provided that it has exactly the concentration c_1; S_2 will become transmitting when acted upon by A alone at a concentration c_2, etc. Now if we produce the influence pattern $(a_i a_k)$ alone, t seconds after its production the distribution of the concentration of A will be again given by the curve ik. But that means that just at that moment all the S-parts between a_i, a_k, and e_r will become transmittent. A reaction e_r will follow the pattern $(a_i a_k)$ exactly t seconds after the influencing. At any other time before or after t, the distribution of A will be no more given by the curve ik. Therefore all the S-parts can become *simultaneously*

transmittent only t seconds after the influencing. Similarly, if we now produce another influence pattern by either reversing the order so as to give $(a_k a_i)$, [curve *ki*], or influencing a_k and a_i simultaneously, or adding a third influence a_n, or simply by varying the interval of time between a_i and a_k, the distribution of the concentration of A will be quite different. At *no time* will it be exactly the same as *ik*, except for very special and exceptional cases. The system L can discriminate various influence patterns.

Summarizing, we see that our system L has the following properties. If a neutral influence or influence pattern is applied, and if this is followed by an unconditioned influence, and if the procedure is applied a sufficient number of times, the particular pattern causes a reaction in e_r, which we may call a conditioned reaction. Application of a conditioned influence pattern without reinforcement by unconditioned stimulation causes the disappearance of the conditioned reaction. The conditioned pattern must be always applied *before* the unconditioned influence. If, during the process of conditioning, we apply the unconditioned influence t seconds after the conditioned, then, after the conditioned reaction is formed, it occurs also t seconds after the application of the conditioned influence patterns.[4]

The equilibrium of a system whose free energy has several minima is stable only insofar as the disturbances which tend to displace it from that equilibrium do not exceed a certain value. Only then will the system always return to the old configuration after the removal of the disturbance. Otherwise, it may pass into an alternative state of equilibrium.

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[4]It is very important to emphasize the following: the above scheme of S-parts being represented by colloids of a special kind, and being influenced by the A and E substances as parameters α and β is only a possible illustration of the general abstract scheme. Generally we may speak instead of fields of concentration of any other field, generated by the influence pattern. It may be an elastic or magnetic field or a temperature field, or a field of any other continuously distributing physical quantity, which acts as parameter α on the S-systems, whose nature, as already said, may also be of any kind. The essential condition for pattern discrimination is the property of the S-system to "conditionally" respond to definite, prescribed amounts of α, and furthermore the linear arrangement of the S-systems, which require that all S-systems should become transmittent simultaneously in order to cause a reaction at all. The introduction of the concept of systems with several equilibria in the explanation of the phenomena of learning, if such an explanation be attempted at all, seems to be unavoidable. In this connection, it may be remarked that a membrane, which changes its polarity, as assumed by H. M. Johnson (7) for the explanation of learning, is only a particular case of a system with two equilibria.

But the state of every thermodynamical system is subject to "spontaneous" fluctuation, due to the molecular structure of the system (5). These fluctuations, which may involve any quantities, such as density, pressure, concentration, etc., are the more pronounced the smaller regions we consider. Statistical mechanics gives us the methods of calculating the probabilities of such fluctuations if the structure of the system and the forces acting upon it are known. If the system which has several equilibria is sufficiently small (a cell, or a colloidal particle), spontaneous transitions from one state of equilibrium into another are possible (19). Since, in general, the different minima of the free energy correspond to different absolute amounts of it, the transition from one state of equilibrium into another will be accompanied by a change of the free energy of the system. According to the laws of statistical mechanics, spontaneous changes will more frequently occur in states with less free energy. Purely thermodynamically, it is equally possible that for S-systems to possess a smaller free energy in the permeable state than in the impermeable, or inversely. Hence some S-systems will have a tendency to remain permeable, some others, on the contrary, will have the tendency to remain impermeable. Accordingly, some established conditioned reactions will be very stable, others will be unstable, and tend to disappear spontaneously. Any strong disturbance, like a strong neutral stimulus, will make them disappear. Stable reactions, on the contrary, even if made to disappear by a strong disturbance, may again appear spontaneously after a while. Due to this difference in stability and to the possibility of spontaneous transitions from one state into another, certain reactions, though transmitted through S-parts, and not through immediate contact of a- and e-parts, will always have the character of unconditioned reactions. We shall call such reaction "proper reaction."

A reaction in one or several e-parts of the system causes, in general, a change in the environment, and this change in its turn may act specifically on some other a-parts, thus causing what we shall call an "inner influence."

Now let our L-system possess amongst its several proper reactions one in particular,

$$(a_i, a_k, a_m, a_n) \rightarrow (e_b, e_h, e_j), \qquad [1]$$

where (a_i, a_k, a_m, a_n) is some influence pattern, composed of the influences a_i, a_k, a_m, a_n; and (e_b, e_h, e_j) is the corresponding reaction pattern. Let e_b, e_h, and e_j cause correspondingly a_b', a_h', a_j' as inner stimuli. Then we have

$$(a_i, a_k, a_m, a_n) \overset{m}{\rightarrow} (a_b', a_h', a_j'), \qquad [2]$$

that is, a certain influence pattern follows necessarily after a certain other influence pattern.

Now let our system be placed in such an environment that it is subject to certain external influence patterns, a^*, which as such are neutral, but which are followed by other unconditioned influences in such a way that a conditioned reaction

$$(a_s^*, a_t^*, a_u^*, a_v^*) \rightarrow (e_b, e_h, e_j) \qquad [3]$$

is established. Since these reactions cause inner influences, we have

$$(a_s^*, a_t^*, a_u^*, a_v^*) \rightarrow (a_b', a_h', a_j') \qquad [4]$$

Hence, different external influences cause the same internal influences (cf. [2] and [4]). But, whereas in the first case the reaction is a proper one, and goes over a stable path, the path transmitting the reaction [4] may or may not be stable.

Under the influence of the spontaneous fluctuations, a number of other conditioned paths will be formed in the system. Amongst the innumerable possibilities there is the possibility that each of the external actions

$$a_s^*, a_t^*, a_u^*, a_v^*$$

will cause correspondingly such a conditioned reaction

$$e_i^*, e_k^*, e_m^*, e_n^*$$

which causes the influences

$$a_i, a_k, a_m, a_n$$

as inner stimuli. So that we have

$$a_s^* \rightarrow e_i^* \rightarrow a_i;\ a_t^* \rightarrow e_k^* \rightarrow a_k;\ a_u^* \rightarrow e_m^* \rightarrow a_m;\ a_v^* \rightarrow e_n^* \rightarrow a_n. \qquad [5]$$

In this case, the external influence pattern $(a_s^*, a_t^*, a_u^*, a_v^*)$ can cause the internal pattern (a_b', a_h', a_j') in two ways: either directly, according to [4], or by causing, first, according to [5], the pattern (a_i, a_k, a_m, a_n), which in its turn will cause (a_b', a_h', a_j'), according to [2], *using the stable proper path.*

As said above, the establishment of a path (the change of permeability of the *s*-systems) is connected with a change of free energy. If now the establishing of the paths [5], which, as said, will have occurred spontaneously after sufficient time, will correspond to a decrease of free energy of the whole system, then the second way, *via proper paths,* will be a more stable one. The first way, expressed by [4], may eventually disappear, the second [5] will persist. Of course, [5] will not be the only *detour* connection which will occur due to statistical fluctuations. But of all possible ones, only the most stable will remain permanently.

Using a somewhat more "psychological" terminology, we may say that the conditioned reaction [3], and the resulting connection [4], which are established by changes of environment, represent the "experience" of the system; whereas the connection [2] is "instinctive" or an earlier established experience, which became fixed by long use. We may then describe the whole phenomenon considered above by saying that the system, by associating the "elements of experience" a^* to certain "older" elements a (Formula 5) interprets the experience [4] in terms of the "innate conception" [2]. How long a time the system will require "to find" the proper interpretation will depend on the probability of certain transitions in the S-systems, and these, in their turn, will depend on the constitution of the whole system. Speaking anthropopsychically, we may also say that the system "tries out" different possible interpretations, until it finds the correct one, characterized by the minima of the free energy. In this connection, it may be mentioned that H. Poincaré emphasized very strongly the "trial" character of mathematical creative thought (11).

In concluding, I wish to express my sincere thanks to Dr. H. M. Johnson for a thorough discussion of this paper and many valuable comments.

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L'APPRENTISSAGE COMME TRAIT CARACTÉRISTIQUE DES SYSTÈMES PHYSIQUES

(Résumé)

L'état d'équilibre d'un système physico-chimique correspond à l'extrême d'une certain quantité thermodynamique: maximum de l'entropie ou minimum de l'énergie libre. Dans cet article on se limite aux considérations des processus isothermiques et emploie donc l'énergie libre. Les relations fonctionnelles entre l'énergie libre et les quantités qui déterminent l'état d'un système sont déterminées par le milieu, et varient avec celui-ci. Un changement du milieu cause un changement du minimum de l'énergie libre et une variation correspondante dans l'équilibre du système. Cependant, tant que l'énergie libre n'a qu'un minimum, le système est toujours dans le même état pour un milieu donné, et une variation donnée du milieu cause toujours même "reaction" dans le système. Si, cependant, l'énergie libre a plusieurs minima, le milieu seul ne détermine pas l'état du système, puisque celui-ci peut maintenant posséder plusieurs configurations d'équilibres pour

le même milieu. Comme l'auteur a montré dans une série de publications antérieures, l'état du système dépend maintenant en grande partie de son histoire antérieure. Pour cette raison la "réaction" aussi du système à un changement donné du milieu dépendra de l'histoire antérieure. On fait l'analyse des différentes possibilités et l'on montre que de tels systèmes avec plusieurs équilibres possèdent nombre de traits caractéristiques importants, caractéristiques des réflexes conditionnels, surtout celui de répondre conditionnellement aux formes temporelles ou spatiales des changements du milieu.

RASHEVSKY

DAS LERNEN ALS EIGENSCHAFT PHYSISCHER SYSTEMEN

(Referat)

Der Gleichgewichtszustand eines physikalisch-chemischen Systems bestimmt sich durch das Extremum von gewissen thermodynamischen Grössen: Maximum der Entropie oder Minimum der freien Energie. Indem wir uns in der vorliegenden Arbeit auf isothermische Vorgänge beschränken, gebrauchen wir hier die freie Energie. Die funktionelle Beziehung zwischen der freien Energie und denjenigen Grössen welche den Zustand des Systems bestimmen, hängt nun auch von der Umgebung des Systems ab, und veränderung der letzteren. Jede Veränderung der Umgebung hat eine Verschiebung des Minimums der freien Energie zur Folge, und bewirkt eine entsprechende Änderung in der Konstitution des Systems. Solange aber die freie Energie nur ein Minimum besitzt, ist der Zustand des Systems durch denjenigen der Umgebung eindeutig bestimmt, und gleiche Veränderungen der Umgebung rufen stets gleiche Veränderungen, "Reaktionen," im System hervor. Hat aber die freie Energie mehrere Minima, so bestimmt die Umgebung den Zustand des Systems nicht mehr eindeutig, weil eben das System jetzt bei der nämlichen Umgebung in verschiedenen Gleichgewichtszuständen verharren kann. Wie Verfasser in einer Reihe von Arbeiten gezeigt hat, wird in diesem Falle der Zustand des Systems auch von der Vorgeschichte abhängen. Es werden also auch die Reaktionen des Systems auf gewisse Veränderungen der Umgebung von der Vorgeschichte abhängen. Die Untersuchung der hier möglichen Fälle zeigt, das solche Systeme mit mehreren Gleichgewichten eine Anzahl von Eigenschaften besitzen, welche für die bedingte Reflexe charakteristisch sind. Besonders interessant ist die Fähigkeit solcher Systeme auf räumliche und zeitliche "Veränderungsgestalten" der Umgebung bedingt anzusprechen.

RASHEVSKY

SHORT ARTICLES AND NOTES

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LASHLEY ON "CEREBRAL CONTROL VERSUS REFLEXOLOGY"

Walter S. Hunter

It is always difficult to decide how far a printed scientific discussion of matters in dispute can go without burdening the literature with needless repetitions, self-justifications, assertions, and counter-assertions. It seems to me possible in the present instance to offer certain comments on Lashley's recent article (4) which will further clarify the issue and which may be of service in the advancement of the science. No serious student of these matters will, of course, base his conclusions concerning the theory of equipotentiality solely upon the arguments offered by Lashley and me. He will go to the experimental literature and evaluate the problem on the basis of a careful analysis of the work there presented.

1. Does Lashley deny the existence of projection areas? We have his present statement (4) that he does not deny them *even in a functional sense.* However, in 1920 he wrote, "Finally the experiments raise another question of fundamental importance. What becomes of the concept of sensory and motor projection areas if the entire cerebral cortex of the rat is equipotential in learning?" (3, p. 103). Again, later in the same paper he writes, "Not only are areas to which associative function might be ascribed unnecessary for the acquirement of habits but even projection areas, which have been considered as hereditarily organized relay stations for impulses to or from the cortex, may be dispensed with without any discoverable loss in ability to learn a difficult reaction which is normally mediated by those areas" (p. 123). In 1917 Lashley and Franz wrote as follows: "The ability of the animals to form habits after the loss of those parts of the brain which are normally used in learning, the rëestablishment of motor control after the loss of the stimulable area of the cortex and of the corpus striatum, and the seeming equipotentiality of the different parts of the frontal lobes in

the functioning of complex habits go far toward establishing the complete functional interchangeability of all parts of the cerebral cortex" (6, p. 133).

That I have not been alone in my belief that equipotentiality is incompatible with the concept of functional projection areas and in my belief that Lashley denied the existence of such area is evidenced by the following statement made in 1926 by C. J. Herrick: "The problem of cerebral function was approached by Franz and Lashley with a definite system of preconceptions based on their former experience with the plasticity and inconstancy of cortical activity in contrast with the rigid mosaic schemes of cortical localization of function hitherto so popular" (1, p. 193). Herrick quotes the above on "complete functional interchangeability of parts" and then writes: "This motif reappears in all of the subsequent reports, but to me it seems that this work in its entirety by no means points in that direction, even in the case of so simple a brain as that of the rat. . . . That there is a functional specificity of some sort corresponding to the known anatomical specificity of the cortex of the rat there can be no reasonable doubt."

2. In his reply to my paper Lashley frequently writes as though I were criticizing the concept of mass action rather than that of equipotentiality. (The fact that I was neither discussing mass action nor neural integration explains many of the shortcomings which he finds in my article and which I therefore need not discuss here.) There is evidently a need for a clearer differentiation of these two terms. Mass action, to me, is not so much a theory as it is a convenient term to designate the finding that efficiency varies with the amount of functional tissue. As opposed to this, equipotentiality means non-differentiation of function. This equipotentiality may be thought of as applying to the whole cerebrum, and it is here that Lashley's maze experiments seem relevant, or it may be thought of only within functionally well-differentiated areas. Thus the cerebrum may have functionally differentiated areas for vision, audition, and the like, and so not be equipotential as a whole; and yet the visual area or the auditory area may show no sub-differentiation of function. The establishment of equipotentiality in this latter and very limited sense will require experiments in which training will be rigorously limited on the stimulus side to certain parts of the receptor. If we are dealing with vision, for example, head- and eye-movements of the subject must be so controlled that the stimulus will affect only a certain part of the retina and thereby involve primarily the cortical projections of this retinal area. In the use of the discrimination box with the rat, no such control is possible. Probably every part of the rat's retina is stimulated by the light as the rat runs through the box. One would not expect therefore that the cortical representation would be confined to a part of the visual area. Rather all parts would be involved in the training, and extirpation experiments would reveal data showing no particular cortical localization of this habit within the visual area, although the area as a whole would be involved.

3. In my presentation of the sensory control of the maze I have not been concerned primarily with experiments showing the rôle of stimuli in the *learning* of the maze so much as I have with experiments showing the rôle of stimuli in the control of the perfected maze habit. The literature cited contains evidence upon both points and clearly indicates the multiple stimulus control of the perfected maze habit. It this is the case, then, in my opinion, the evidence offered by Lashley does not require a modification of our views of the nature of cortical functioning.

4. I feel that some comment should be made on Lashley's reference to my own defense of "some symbolic process or some central neural engram" in connection with the temporal maze. At times he writes as though I had said that my experiments led me to assume a symbolic process in order to explain *maze* behavior; but at other times he specifically and correctly limits this assumption to experiments on the double alternation temporal maze. Now it by no means follows that because a symbolic process is required, if it is, to explain one type of behavior that it is also required to explain another type of behavior. I have been forced to postulate something called the symbolic process in order to account for behavior elicited in circumstances where there are no differential stimuli available for controlling behavior. Such stimuli are available in the ordinary maze experiment, and therefore the hypothesis is not called for in that situation. To be sure, after one has proved that a given animal has the capacity for symbolic processes, the possibility always exists that the animal's behavior may be influenced by them at any time. The probability, however, is not very great, as is evidenced by the fact that though man can think and think clearly he often does neither.

Concerning the soundness of the principle that explanations of behavior should be made in peripheral terms so far as possible before appealing to central processes, it might be worth while to point out that Lashley himself has followed this rule, although he now dissents from it. It was only after he and Ball (5) thought that they had ruled out proprioception and the other senses from the control of the maze habit that they could come to the conclusion that the maze habit is controlled by some "wholly central mechanism." These writers say, "Control experiments indicated that they [the rats] did not fall back upon exteroceptive cues for orientation and that the persistence of the habit can not be referred to the remaining proprioceptive sensitivity. From this it is argued that the maze habit can not be interpreted as a series of kinaesthetic-motor reflexes but must be referred to some intraneural mechanism capable of producing an integrated sequence of movements in the absence of directive sensory cues" (p. 100). It should therefore follow that, if it can be shown, as I think it has been clearly shown, that directive sensory cues are functional in the guidance of the perfected maze habit then the appeal to central processes is unnecessary and scientifically undesirable.

5. Lashey apparently uses the term mass action as equivalent to integration. His statement is as follows: "*c*. The loss of the habit after cerebral lesion is due in large part to the disturbance of some function other than cortical sensory reception—the unknown function which I have called mass action" (4, p. 9). The integrative action of the nervous system is a well-established fact, but we still lack adequate information upon how integration is accomplished. Undoubtedly such factors as facilitation, inhibition, refractory period, and chronaxy are vitally involved. How and to what extent mass of nervous tissue aids or participates in integration remains to be determined. Surely cubic centimeters of cortical tissue will not take us far in the understanding of the process.

Lashley's experiments give abundant evidence of the integrative action of the nervous system. This comes out clearly in the case of the blind rats who learned a maze and who then suffered a loss of the habit consequent upon an occipital lesion. It is also evident when comparisons are made between the amount of loss of the habit which results from the elimination of a sense organ, on the one hand, and the elimination of the corresponding projection area, on the other hand. Where a portion of the cortex is removed, and so where integration is interfered with, the loss is greater than where the cortex is left intact and the receptor is removed. These facts, however, bear neither upon equipotentiality nor upon mass action (unless mass action equals integration). They are evidence for the existence of integrative phenomena, a matter which is not in dispute.

Attention should be called specifically to the following statement: "On the basis of the evidence reviewed above, I must again conclude that the *theory of sense privation* as an explanation of the relation between the extent of lesion and deficiency of performance is inadequate to account for the facts observed. The hypothesis which seems best in accord with the experimental results is that, for certain types of behavior, the *projection areas have, in addition to their specific sensory or motor functions, a nonspecific* (perhaps facilitative) *function* in which they are equipotential" (4, p. 13). (All italics mine).

Were it not for the last word, equipotential, I should be in thorough agreement with the statement. My emphasis upon the multiple stimulus control of the maze habit (2) in no way stated or was meant to imply that integration did not exist. I was concerned to show that the work on the maze habit did not justify the view that the cortex is equipotential. From the above statement I would assume that *Lashley is now presenting no new theory of neural action,* unless he can show that integration is equipotential and that such a conception has meaning. Rather we have the widely accepted view that the cortex functions both in integrative ways and by means of functional areas.

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COMPARATIVE BEHAVIOR ON DIFFERENT TYPES OF MAZES[1]

RICHARD W. HUSBAND

Are we justified in generalizing broadly the results obtained from research with a single piece of apparatus or a given set of conditions? We frequently find in the literature such statements as this: "It is evident that in motor lines at least, persons learn most rapidly when. . . . . " Whether we may be sure that results hold, even for all types of motor activity, is a serious open question.

Intercorrelations between memories for different types of material were found by Bennett (1) to be very low; and recently R. H. Seashore (8) has found reason to doubt that there is a basic motor capacity. There is low correlation between learning of different tasks and between learning and intelligence. In fact, correlations between performance on two mazes of generally similar construction have been frequently disappointingly low.

Consequently, the writer, while in the course of a rather extensive investigation of methods of learning, was confronted with the serious question as to whether generalizing the results obtained from only one type of maze would be safe. So it was thought advisable to study learning on several other types of pattern, to see how general was the learning behavior shown on the maze on which the main body of experimentation was conducted. Accordingly, three other mazes representative of as many types of construction were chosen.

[1]This work was done at Stanford University under the supervision of Professor W. R. Miles. The writer wishes particularly to thank Dr. Miles for the privilege of using his X-form maze prior to its description in the literature.

The Method

The four mazes on which behavior is compared were as follows: (*a*) The Warden U-maze (10), of ten turns, built in a stylus form (see Knotts and Miles, 4), which was used by Knotts in comparing blind and sighted children (3). (*b*) The same pattern, but in a high-relief mode of construction instead of stylus (Miles, 6). (*c*) The same succession of choices was built in a high relief type of construction, with the units in the Miles X-form (7). Each unit was a square with the diagonals in one linear direction. Thus the learner had the choice of right or left. One side was open and the other blocked, thus forming a blind. Traversing each unit brought him back to the median line of the maze. The highly symmetrical nature of this design and the fact that successive choices to the right or left does not displace one from the median line are the outstanding features. (*d*) The Stone Multiple-T pattern copied from the original rat maze (9) and made into a high-relief finger maze. For picture of the human pattern see Knotts and Miles (4), Figure 2. This maze has twelve blinds instead of ten as the other patterns had, and the turns were in all four directions instead of just right and left. Thus it has a more irregular design than the others.

These mazes will be termed S, F, X, and T, respectively. The patterns, together with the numbers of the individual blinds and the entrances made into them are illustrated in Figure 1.

Each of the four groups was composed of 20 subjects taken from classes in elementary psychology. Instructions were given the subjects before they started learning as to the general nature of the mazes, and the particular manner of construction of the one they were to learn. This was carefully worded so as not to suggest any particular method of attack. Trials were immediately successive. The criterion was three perfect, or three perfect out of four trials in succession.

After learning had been completed, the subjects were asked to describe the method by which they had learned the pattern. The time of initial application of the successful method, possible discard of other approaches, and points of special difficulty were gone into also.

Results

The main features of the treatment of results will be as follows: (*a*) the comparative difficulty of the different maze patterns; (*b*) comparisons of the different methods of learning; and (*c*) the effect of the use of these methods on performance on the various mazes.

1. *Trials to Learn.* The figures on trials, in medians, means, quartiles, and ranges, are given in Table 1. There were two unfinished cases in the stylus task, and one in X. Ten trials were arbitrarily added to the number already done by these subjects in order to compute the means, although it

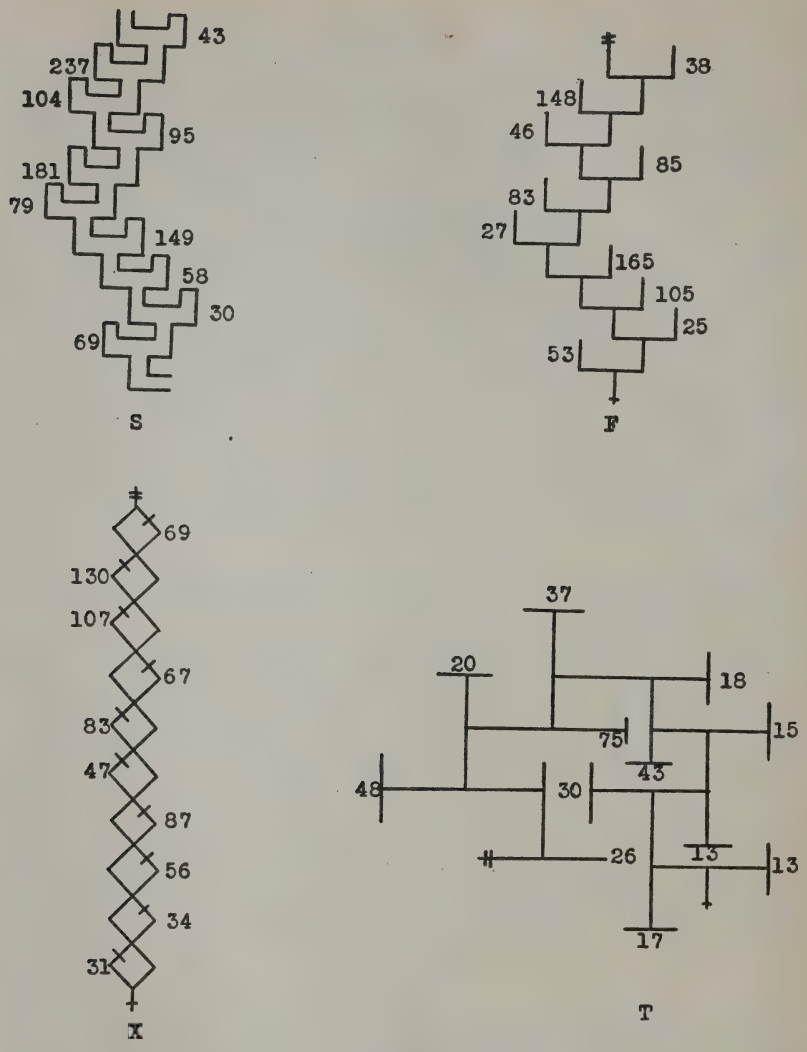

FIGURE 1

MAZE PATTERNS USED, WITH TOTAL NUMBER OF ERRORS MADE ON EACH BLIND

is doubtful if all would have finished within this limit. The medians were, of course, not affected by these cases.

The stylus maze, S, was harder than F, the high-relief maze which was

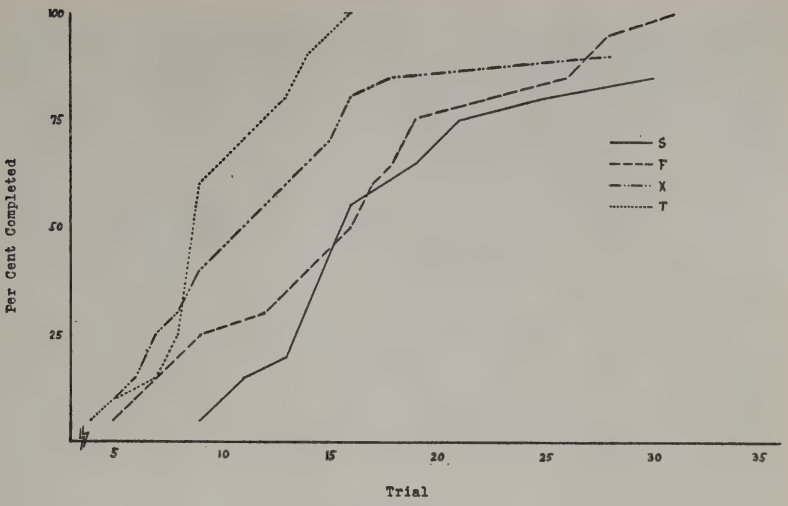

FIGURE 2

SUMMATION CURVES OF TRIALS TO LEARN FOR THE FOUR MAZES

explored directly by finger contact, but not as much harder as Warden's results (10) would indicate. Possibly the fairly complete directions gave an earlier orientation. The lack of such an orientation appears to be the greatest point of difficulty in maze learning. Otherwise the two techniques appear alike, and there was just the same task to learn, as far as sequence of turns, proportions, etc., go.

The X-design was expected to be somewhat harder than F, but it turned out to have practically the same difficulty. This may be explained on the basis that it is so regular in design that most subjects realize very quickly that they will have to count the turns. Spontaneous remarks as well as answers to questions prove this.

The distributions of the scores, in addition to means, are very instructive. The latter may be equal for two groups, yet one may be widely distributed, while the other may be closely grouped. The distributions in the form of

TABLE 1

TRIALS TO LEARN DIFFERENT TYPES OF MAZES

	S	F	X	T
Median	16.5	17.0	12.0	9.8
Mean	23.1	16.7	16.1	10.0
First quartile	15.2	10.5	7.5	8.5
Third quartile	22.5	22.5	16.5	12.5
Range	9-76	5-31	5-51	4-16
S.D.	16.6	7.5	14.3	4.0

summative curves for the four maze patterns are given in Figure 2. S is solved rather later than F or X, but has a bunching (shown by the sharp rise) around the 15th and 16th trials. With X a rather unique fact is shown; there are very few cases around the actual mean. Subjects using efficient methods learn very quickly, while others find it nearly impossible. T is very easy; one subject in fact made perfect performances after the first trial. This maze is probably too easy for regular experimentation with humans, although it is of ideal difficulty for rat learning programs. F shows a more even spread than the others. Only two subjects learned in exceptionally short times, and none required any great number of trials to complete learning.

2. *Errors.* The rankings and general proportions of errors agree very well with the figures on trials to learn, given in Table 1. We see in Table 2 that T is again by far the easiest, X and F are practically equal in difficulty, and S is found considerably harder than the others. The same holds true with the ranges, except at the higher end of the X distribution, where a pure motor method is at a severe disadvantage.

TABLE 2

TOTALS OF ERRORS AND TIME FOR THE VARIOUS MAZES

	S	F	X	T
Errors	54.5	38.7	36.6	20.0
Time (seconds)	736	526	530	358

The error curves, given in Figure 3, illustrate the progress of learning on the various patterns. These are plotted on semi-logarithmic paper, which portrays equal relative increments of progress as equal. That for X is clearly below S and F, patterns having the same sequence of turns, due to early solution on the part of the strict counters. However, because of the slowness on the part of the motor learners with X, the curve does not meet the base line as early as does that of F. The curve for T shows again the extreme ease with which this pattern is solved.

The individual errors may be compared directly for all but the T-maze, since the other three patterns were composed of exactly the same sequences of turns. There is a fairly good agreement among error totals, and easy and difficult blinds are about the same on all patterns. The piling up of errors in a sequence, noted previously by the writer (2), is seen in all the types of patterns. It is minimized in the X-design, and somewhat intensified in the stylus task. In X lateral spatial elements are practically eliminated while in S the mode of contact causes this lateral spatial element to play more part than with direct finger contact. Errors on T are fairly constant from one blind to another, the most frequently entered alleys being turns inward. The general direction of the true path is circular, and subjects tend to shorten the process.

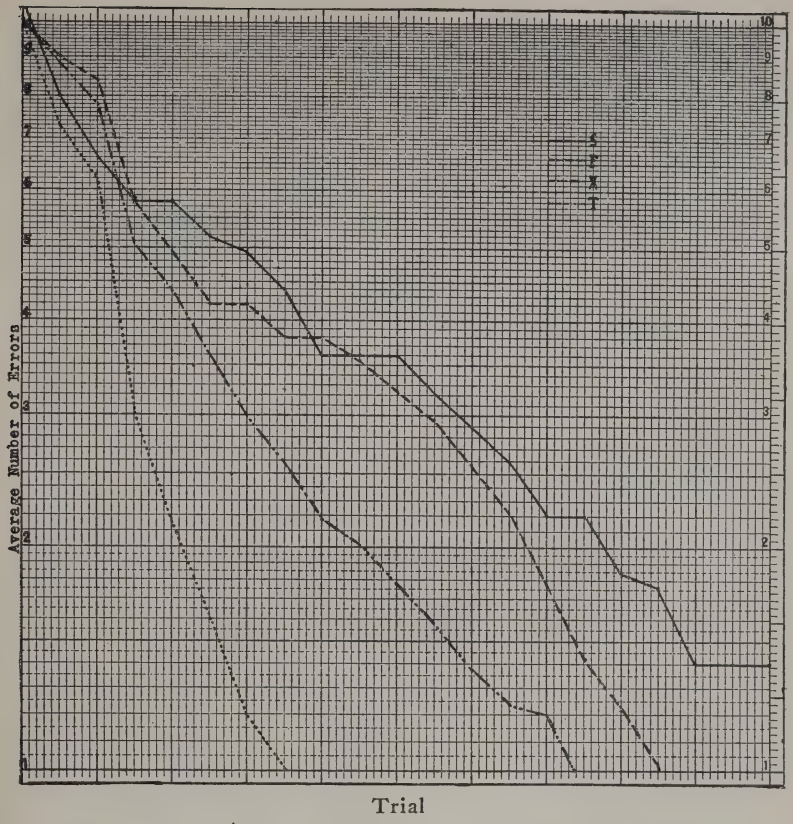

FIGURE 3

ERROR CURVES FOR THE FOUR MAZES

3. *Time.* The total time consumed, listed in Table 2 along with errors, agrees perfectly with the error scores. We notice that in all three scores, trials, errors, and time, S is the most difficult, X and F are practically equal in difficulty, and T is by far the easiest.

4. *Use of Methods.* As a result of questioning the subjects after learning had been completed, it was found that the following methods had been used in attacking and solving the problem. (*a*) Counting; a verbal or abstract scheme. The subject learns the turns absolutely, usually by some scheme based on counting the number of right- and left-hand turns in order. (*b*) Motor or kinaesthetic method. This procedure is entirely automatic

and unconscious, learning being effected purely by motor and muscular cues. (*c*) Visual method; a mental picture of the layout of pathways is formed. There are also numerous combinations of two or all three of these basic methods, of which the only one having enough cases to make reliable results is (*d*) verbal procedure with some motor aid. Subsequently these methods will be referred to by the above letters.

We see from Table 3 that different constructions of pattern bring out different types of learning attacks. The most outstanding feature is the high percentage of the X-group who learned by a strict counting procedure.

TABLE 3

LEARNING METHODS EMPLOYED FOR THE DIFFERENT MAZES

Method	S	F	X	T
a. Counting	5	8	13	3
b. Motor	3	6	2	5
c. Visual	3	–	–	5
d. Count; motor aid	5	2	1	3
e. Miscellaneous	4	4	4	4

We also notice that S brings out more motor and less verbal means of learning than the same pattern, F, in the finger design. T, which was comparatively irregular, was rather difficult to verbalize, but here kinaesthetic procedures were not as handicapped as in the more regularly designed mazes.

5. *Description of Behavior on the Various Mazes.* From the above table we see that the proportions of those using each method vary considerably depending on the type of maze pattern used. Actually, then, what differences in the manner of learning are produced? How does the average subject learn? Why are some methods particularly handicapped?

a. Stylus. General behavior was decidedly motor on this pattern. The stylus situation seemed very confusing to the subject; there were many retracings, multiple entrances, corrections, and other deviations from simply going straight through from start to finish. The greatest source of confusion arises in distinguishing a blind alley from a correct choice. Often when coming out the correct side the subject will hit the opposite wall of the next unit, and then retrace, without realizing that he has a new right-left choice before him. Then he finds a real blind out the other side, and is naturally greatly confused. This situation would upset the confidence of the subject, make him doubtful of the accuracy of his count, and thus delay his final perfection. In general, it may be said that complete orientation to the situation was delayed, in comparison with adjustment to the high-relief type of construction, and that learning was slightly slower even after orientation had taken place. Very few attempted visualization on this pattern; it seemed somewhat more difficult.

b. Finger. About two-thirds of the subjects learned by the counting method, either in whole or in part. A few used the kinaesthetic or motor approach, but this is far less efficient than the former. No subject learned visually, although the writer has found an occasional one who learned similar patterns in this manner.

c. X-Type. Of the 20 subjects, 14 learned this pattern by a strict counting method which started early in learning. This procedure practically suggested itself because of the absolute regularity of the units. Many apparently realized even during the preliminary instructions that this approach would have to be taken. This early realization that counting was necessary appeared very rarely in the finger or stylus patterns, possibly because the trends which appeared gave the subject an idea that general sense of direction would be sufficient. Motor learning was a great handicap, only two subjects using it in pure form. One failed and the other took three times the average number of trials to learn. Several learned a few turns in a motor manner, but relied chiefly on counting.

d. T-Type. Behavior on this pattern was entirely different from that on the previously discussed mazes. This resulted from the choices being forward and back as well as right and left, and because the true path leads in a general circular direction, instead of in a more or less straight line away from the subject. As a rule solution was so easy that no great concentration was necessary. This resulted in only three learning it in a verbal manner. It was further found more difficult to put into operation a verbal attack on this more complex pattern. Retracing during the first trial or two was fairly common. General sense of direction was sufficient to enable about 10 of the 12 choices to be made correctly, and remembrance of the remaining choices was about all the verbal aid necessary to complete learning. This condition made the T-maze very easy to learn. The average difficulty was about two-thirds that of the others, although it had two more blinds. Visual means were much more common, as it was found that a fourth of the subjects professed to have gotten a picture of the correct pathways. In contrast to the first and last turns being learned most easily in the other three patterns, the order of elimination on this seemed to be progressive.

6. *Comparative Efficiency of the Different Methods.* Figures are presented in Table 4 for those using the four principal attacks on the different mazes.

The T-maze is too easily learned to differentiate between the various methods of learning. All appear about equal in efficiency. However, the other three mazes agree very closely. In every case we see that the verbal or counting procedure has a pronounced advantage over the other modes of attack. The motor method is consistently poorest, and especially handicapped on the X-design. This is probably accounted for by the fact that

TABLE 4

THE INFLUENCE OF LEARNING METHODS ON TRIALS, ERRORS, AND TIME FOR THE FOUR MAZES

		S	F	X	T
1. *Trials*	Average	23.1	16.7	16.1	10.0
	Count	14.0	9.8	10.1	9.3
	Motor	52.0	23.0	55.0	9.2
	Visual	27.3	–	–	11.4
	Count, motor aid	16.8	–	–	–
2. *Errors*	Average	55	39	37	20
	Count	28	18	20	18
	Motor	151	54	151	17
	Visual	61	–	–	20
	Count, motor aid	29	–	–	–
3. *Time*	Average	736	526	530	358
	Count	490	294	327	323
	Motor	1139	793	1814	385
	Visual	1007	–	–	352
	Count, motor aid	494	–	–	–

this design is such that spatial cues are minimized. Of the three patterns the stylus was the only one which several subjects learned by visual means, and on this it was rather inefficient, although not so much as the motor attack.

7. *Correlations with Intelligence.* Another feature which might lead to the adoption or rejection of a proposed maze is its correlation with intelligence scores. Correlations are given between total time, which the writer found to correlate the highest of the three variables, and scores on the Thorndike Intelligence Test. They are as follows: S = .77; F = .43; X = .63; T = .11.

Considered as one possible unit of a test battery, the figures for the S and X mazes are high. In X the attacks on the part of the subjects were more homogeneous than on the other patterns, which contributes to higher correlations, as shown in another article (3). S is correlated somewhat higher than F, probably because of its being harder, and consequently it provides a better sampling of ability. The correlation of performance on T with intelligence is almost negligible, undoubtedly due to its extreme ease of solution. It fails to properly differentiate subjects in terms of their real abilities.

CONCLUSIONS

If we ask ourselves what maze pattern is best for practical use, the answer naturally depends somewhat on the purpose of the experiment. To show up the contrast between the two chief methods of learning, verbal and motor, F is probably the best. Both methods can be used, and neither is unduly handicapped.

For a motor act T or S seem suitable. A combination of the features of the two, in having irregularly arranged alleys together with stylus construction, would probably intensify this feature. Many mazes used in the past have been just of this type and their users on this basis have been justified in terming maze learning a *motor* act. The present writer is, however, convinced that with properly designed patterns the act need not be at all motor in nature.

If ideational learning with practical elimination of motor and visual elements is desired, X is ideal. A few cases might have to be discarded, but the great majority learn this type of maze in purely ideational terms. In fact, Professor Miles designed this type to get away as far as possible from the feature of motor learning.

Summary

1. Human behavior was studied with mazes of various types of construction: three degrees of regularity of design, and two different types of contact were employed. The relative difficulties of these mazes and the types of behavior brought out by them are compared. The relative efficiencies of the different methods on each are also discussed.

2. The stylus mode of gaining contact proved to be the most difficult to master; one with a finger contact and a rambling design the easiest. The two others used both had finger contact, but one was of a medium degree of regularity and the other was highly regular and with spatial cues minimized.

3. The more regular and homogeneous the design, the more verbal and abstract the behavior becomes; the more irregular it is, the larger the percentage of individuals learning by motor means. Stylus contact produced behavior more typically motor than appeared in the same pattern in a raised finger design.

4. Outside of the irregularly designed pattern, which was too easy to differentiate the methods in terms of efficiency, the verbal method proved far the best in all patterns. The motor attack was the least efficient. It was especially handicapped in the highly regular designs. A set of choices in an approximate circle or spiral tends to be more easily mastered in terms of motor experience than if the same sequence of choices is arranged in a nearly straight line.

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ILLUSION IN THE CASE OF SUBLIMINAL VISUAL STIMULATION[1]

JOSEPH BRESSLER

Do stimuli of which we are not conscious have any effect in determining our responses? More specifically, would something that we could not see, although present and found to be acting upon the retina of the eye, evoke a response that would be comparable if not in magnitude at least in quality to a response to a similar but visible stimulus? And if such a response occurs, what relationship will its magnitude bear to the strength of the stimulus?

In 1900, Knight Dunlap (2) published the results of an experiment, the purpose of which was to discover whether subliminal visual stimuli had any effect on the judgment of distance. Dunlap used a modification of the well-known Müller-Lyer illusion in which he replaced the visible wings at the ends of the line that was to be measured with wings of equal dimensions but which, under the conditions of the experiment, were invisible to the observer. Dunlap assumed that if the invisible wings exercised any motive power at all, their effect upon the observer's judgment of distance could be measured by comparing his judgments on the length of the line with the wings with those on the length of a line without them. Thus, if the subliminal stimulus did motivate the response, the observer would judge the line bearing the wings longer than a line differing from the first only in not possessing wings. That is exactly what Dunlap found. The dif-

[1]The experiment described in this paper was carried out under the direction of Professor H. L. Hollingworth, and the writer hereby acknowledges his guidance and assistance.

ferences, to be sure, were very small and deceived Dunlap into exercising excessive caution in formulating his conclusions. Because the invisible wings did not create as great an illusion as clearly visible ones, irrelevant and inconsequential causes were suggested to explain the seeming discrepancy. In fact, the experimenter was prompted to doubt the efficacy of his apparatus and to conclude that his results were suggestive but required the verification of further experimentation.

In 1907, Titchener and Pyle (4) repeated the experiment with every effort to duplicate the conditions. They found that imperceptible shadows have no effect on the judgment of distance. In their discussion of Dunlap's results, they also were influenced by the small size of the effect of the invisible wings and were inclined to attribute it to chance. Consequently, they strove to eliminate the factor of chance by giving the subject an abundance of preliminary trials until his responses were as stable as possible. We know that practice sometimes reduces the size of the illusion when the visible wings are used in the customary Müller-Lyer illusion experiment. Why might not the practice effect have been operative in this experiment with imperceptible wings and consequently so minimized their effect as to have made it incommensurable with the measuring device used? With further reference to the small effect obtained in Dunlap's research, Titchener and Pyle ask the question, "Is it not more reasonable to suppose that an illusion motive, if effective at all, will be effective in full strength?" The antithetical results of the previous investigators and the desire to answer this question led us to repeat the experiment.

In performing the experiment we decided not to use Dunlap's rather cumbersome apparatus but one similar in some respects to that used by a student of Professor Hollingworth (3). He used two cards, each bearing a dark base line of equal dimensions. On one of these cards he drew very faint arrows pointing in at the ends of the base line, and on the other similar arrows pointing out. He then placed the subject at such a distance where the arrows would not be visible and instructed him to indicate which of the two base lines was longer.

Needless to say, the method he employed in constructing his cards does not lend itself readily to repetition because of the possible inaccuracy in duplicating the stimulating power of the lines and because of the incommensurability of the light intensity of the imperceptible arrows. We have, therefore, used material that partially eliminates these almost insurmountable difficulties.

First we procured a set of the Hering Gray Papers. We then chose one of these (No. 16) as a background upon which we pasted horizontally a strip of black paper (No. 46) measuring 7 cms. long and 3 mm. wide. With the use of this slide, which will be referred to as Slide O, we later measured our subjects' judgments of the length of the 7-cm. line it contained. We now

had two further problems; one, to construct a subliminal stimulus whose effect on the subjects' judgment we were to discover; and second, to make several subliminal stimuli differing in intensity. More specifically, we wanted arrows at the ends of the lines to be judged which would differ in light intensity and which the subjects would be unable to perceive consciously. We therefore chose arrows made from papers 17, 18, and 19, which differed somewhat in darkness from paper 16 and consequently should be barely visible against a background of 16, the extent of their visibility depending directly on the amount of difference between them; the greater the difference, the greater the visibility. Since the papers differed only slightly in darkness, we assumed that under the conditions of the experiment they would have an effect on the subject's retina and at the same time be invisible to him.

Desiring to test our assumption and to express the light intensities of our different arrows in more exact terms than merely stating the numbers of the grays from which they were made, we performed a preliminary test on fourteen subjects to discover their subjective judgments of the differences in intensity of the four papers, 16, 17, 18, and 19.[2] The procedure was quite simple. The subject was shown the papers in pairs and was instructed to indicate which of the two was darker. Each of the papers was compared with 16 until 25 judgments on each were taken.

Curiously enough, we found that paper 19 was not the darkest of the four shades of gray, a discovery which confirmed an anticipated conclusion arrived at from a previous close inspection of the papers. By calculating the percentages of the total number of times the other papers were judged different from 16, we obtained the following results:

Paper	% different from No. 16
19	63
17	65
18	79

On consultation of the Cattell-Fechner Table for determining the *P.E.* from the percentage of right cases and the amount of difference between two stimuli, it will be seen that the difference between 19 and 16 is .49 units; between 17 and 16, .57 units; and between 18 and 16, 1.20 units. These figures can be called the stimulating strength of the respective sets of arrows. Since a stimulus must have a strength of at least 1.00 units to be liminal, it is clear that arrows 19 and 17 were found objectively to be distinctly subliminal. At the same time we can be reasonably sure that they

[2]A detailed account of this experiment with full records for each subject, and measures of reliability, is on file in the library of Columbia University, in the writer's Master's Essay entitled "Illusion in the Case of Subliminal Visual Stimulation," May, 1930.

had an effect on the retina, since the subjects, though at no time conscious that they were making a proper choice, nevertheless detected a difference between the papers. Similarly, paper 18 had an effect on the retina, although it differed from the others in that it should have been supraliminal, as determined mathematically. We know, however, from the subjects' own reports that they were below the threshold since they were not seen. As an explanation, we can say only that the attention of the subjects was so concentrated on the comparison of the lengths of the base lines that they did not see the arrows whose stimulating power was only slightly above the threshold.

Our first aim, namely, to construct and measure subliminal stimuli, was accomplished. However, instead of having three different intensities as we had expected, we had but two; one represented by arrows made from paper 18 and the other by arrows from papers 17 and 19, which were so nearly alike. With these stimuli we proceeded to perform our major experiment, a full description of which will be found in the M.A. thesis (see footnote 2). Briefly, we made several slides in all respects similar to Slide O (described above) except that at the ends of the base line of one we pasted the arrows (pointing out) made from paper 19. This slide will be called A. At the ends of the base lines of two others, we pasted the arrows 17 and 18, the former being Slide B and the latter, Slide C. On still another slide we pasted black arrows which were as clearly visible as the base line. We tested the subjects with this (Slide M) at the end of the experiment to discover whether our subjects succumbed to the influence of the clear illusion motive. The subliminal stimulus could hardly be expected to evoke a response if a clearly visible one did not have that effect. We now constructed a slide holder and measuring device, with which the subject's judgment of the length of each of the lines could be recorded. The three slides bearing the subliminal arrows and Slide O were presented in a mixed order until 24 judgments on each were taken. The subject was then asked to draw what he saw in the slides. Invariably, the drawing was that of an ordinary black line. To eliminate any possibility of being misunderstood, we questioned each subject as to the appearance of the slides. None had any suspicion of the presence of the arrows, but each thought that he was judging lines that differed slightly in length, just as in the previous experiment he had judged papers that differed slightly in color. Following this we had him judge line M until a sufficient number of judgments to insure an accurate average were recorded.

A sample is shown in Table 1 of the complete record of the first subject, such as was compiled for each of the 14 subjects.

Our slides, it will be remembered, all contained lines of exactly equal length, and therefore if the subliminal stimuli present on three of them were not effective in creating an illusion, the subjects would respond equally to all

TABLE 1

AVERAGES, AVERAGE DEVIATIONS, AND THE RELIABILITY OF THE AVERAGES, ALL IN TERMS OF CENTIMETERS

S	No. of trials	Slide	In Av.	In A.D.	σ_{av}	Out Av.	Out A.D.	σ_{av}	Combined Av.
1	24	O	6.87	.14	.029	6.73	.13	.027	6.80
	24	B	7.02	.13	.027	6.91	.11	.022	6.96
	24	C	7.03	.11	.022	6.87	.11	.022	6.95
	24	A	6.98	.11	.022	6.79	.08	.016	6.89
	20	M	7.41	.10	.022	7.36	.08	.018	7.38

four. Their judgment on the length of the line in Slide O would therefore be equal to those on lines A, B, and C. If, however, the invisible arrows did exercise motive power, then the subjects would judge the lines A, B, and C longer than O. That is what happened almost invariably. All the subjects but one consistently judged lines A, B, and C longer than O. Moreover, the differences were found to be for the most part statistically reliable. That is, the differences were great enough not to be attributable to chance but to the actual motivation of the subliminal stimuli. We may conclude, therefore, that the arrows that were at all times invisible to the observers, nevertheless exercised sufficient power to act as illusion motives and cause the subjects to misjudge the lengths of the line at the ends of which they were present.

Furthermore, we found that the responses which the imperceptible arrows evoked were not equal in magnitude; but that the stronger stimulus evoked the greater response. Not only did the subjects judge line C longer than O, but consistently judged it longer than A or B. Again, the lines A and B, which had arrows of approximately equal stimulating strength, were judged approximately equal in length. And yet again, line M, containing as it did the clearly visible arrows, was judged longer than any of the others. The relationships between the various judgments can best be expressed in tabular form and with the aid of graphic illustration. In this table (Table 2) the stimulus will be expressed in *D/P.E.* units as obtained from the Cattell-Fechner Table and the response by the differences obtained when the average of the total number of trials on each of the other slides. There are three averages for each slide; the first, or *In* average, obtained when the variable slide is moved inward to equate the two lines; the second, *Out* average, obtained when the variable slide is moved outward; and the third, the combined average of both *Ins* and *Outs*. In the case of the visible arrows, we must assume that they have a stimulating power of at least 99% and a *D/P.E.* of approximately 3.50.

On the graph, Figure 1, along the Y-axis are plotted the stimuli and on the X-axis the responses (the differences of the combined averages).

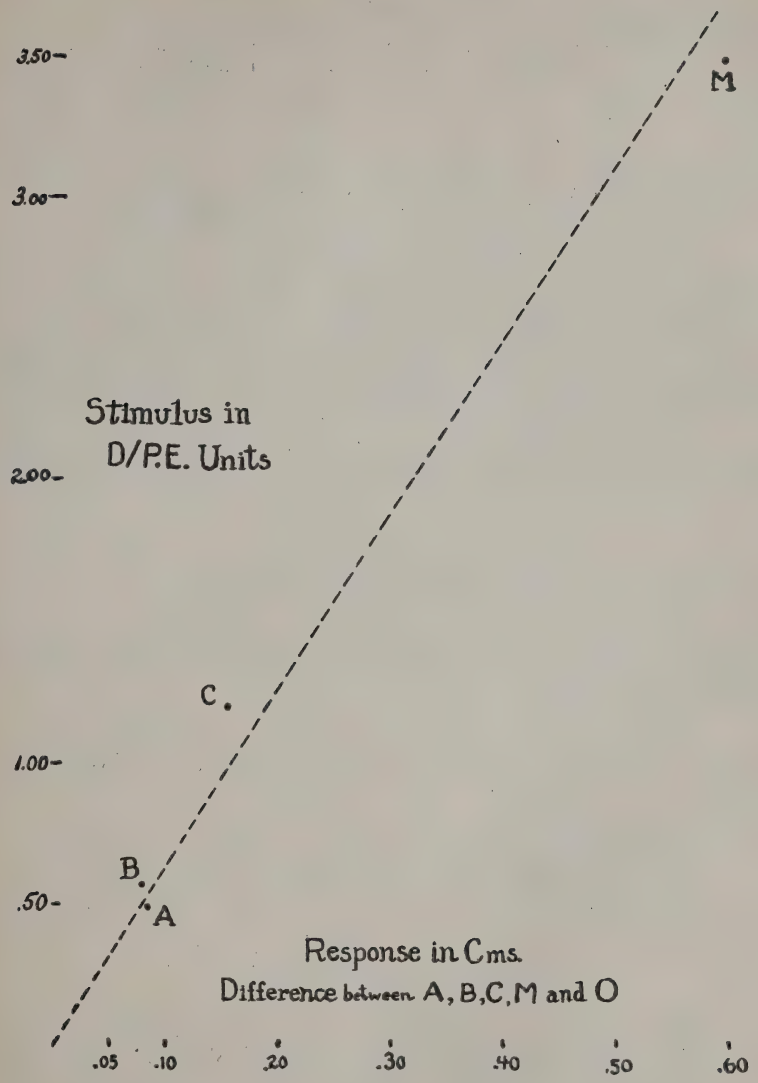

FIGURE 1

It will be seen at a glance that there is an exceedingly close correspondence between the strength of stimulus and the magnitude of the response. We are now prepared to answer the question that prompted us to undertake this

TABLE 2

Slide	Stimulus		Response in cms.		
	% Diff.	*D/P.E.*	In	Out	Comb.
A	63	.49	.062	.115	.085
B	65	.57	.069	.101	.080
C	79	1.20	.153	.161	.155
M	99	3.50	.546	.643	.591

piece of research. Contrary to the inference that the form of the question intended that we draw, we find that illusion motives of unequal strength are not equally effective in evoking a response. We find, instead, that there can be different strengths of illusion motives and that their effect depends directly upon their strength. At least, this is true of the two different intensities that we used in our experiment and also for the maximal stimulus (the slide with the black arrows) used. They are sufficient, at any rate, to indicate that a response follows no "all-or-nothing" law, but rather suggests that it increases gradually as the strength of the stimulus increases.

Summary and Conclusions

1. By means of a psychophysical test, the stimulating strength of three sets of arrows were measured and found to be subliminal.

2. Two of these sets of imperceptible arrows were found to be of equal stimulating strength and the third set to be stronger than the former but still under the threshold.

3. The responses to these stimuli were measured.

4. We found that the Müller-Lyer illusion is effective in evoking a response even when the arrows are so faint that they are not visible to the observer.

5. The magnitude of this response varies directly with the strength of the stimulus, as determined with the customary psychophysical tests.

6. The illusion created by the imperceptible arrows was smaller than that created by clearly visible arrows in proportion to the intensity of the stimulus.

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A STUDY OF LEARNING CURVES: III. THE RELATIONSHIP BETWEEN A GROWTH CURVE AND THE ARC COTANGENT FUNCTION[1]

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In a previous paper (7) we have shown that the hyperbola is an approximation of the *arc cot* function in the region to the right of the inflection point in the latter where an approximation is at all conceivable. In this paper we will show that the arc cotangent function itself is an approximation of another, rational curve which we call a "growth" curve.

By assuming that the rate of improvement in any function with respect to the amount of practice is proportional to the amount already learned up to any specific practice level and the amount yet to be learned, we may write the differential

$$\frac{dy}{dx} = ky\ (l—y) \qquad [1]$$

where x is the amount of practice measured in any acceptable unit, y is the attainment in terms of the number of successful acts per unit time, l the limit of attainment and k a constant or parameter depending upon the individual and the problem.[2]

Multiplying by $y(l—y)$

$$dx = \frac{dy}{ky(l—y)} = \frac{1}{k}\left(\frac{A}{y} + \frac{B}{l—y}\right)dy \qquad [2]$$

From which both B and $A = \frac{1}{l}$

Substituting

$$dx = \frac{1}{k}\left(\frac{1}{ly} + \frac{1}{l(l—y)}\right)dy$$

and integrating,

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[1]This study was made possible through a grant from the Graduate School of Ohio State University.

[2]It would be more accurate to say "upon the individual and the *unit* problem," for undoubtedly in any problem, aside from the very simplest motor skills, the accumulated practice brings about a shift in posture which entirely changes the problem. That the "limit of learning" shares in this shift is obvious so that the limit for the initial stages in any learning problem may not be the limit for the later stages. A single mathematical function will fit only that portion of the learning curve for which the conditions are the same; that is, k and l remain constant.

$$x = \frac{1}{kl}\left[log\ y — log\ (l — y) + log\ c \right]$$

where c is the constant of integration and may be evaluated in the usual way

$$log\ c = log \frac{l — y_0}{y_0}$$

substituting

$$x = \frac{1}{kl} log \frac{y(l — y_0)}{(l — y) y_0}$$

or

$$x = \frac{1}{kl} \frac{cy}{l — y}$$

$$(l — y)\, e^{klx} = cy$$

$$y = \frac{l e^{klx}}{c + e^{klx}} \qquad [3]$$

where e is the Naperian base.

This curve, starting from $y = 0$ rises at an ever increasing rate through an inflection point at $\frac{l}{2}$ and then symmetrically at an ever decreasing rate to a limit at l. This description of the form of the curve is also a description of the chief characteristics of learning curves, particularly those for complex processes [Chapman and Hill (1); Hull (4); et al].

Equation 3 was used in a modified form by Pearl and his associates in their early work on the prediction of population and related problems in the growth of organisms and political organizations, and has been superseded by another form which does not possess the disadvantage of right and left symmetry about the inflection point. Such symmetry presupposes, as Pearl (6) points out, that whatever factors are inhibiting growth in the initial phases must be numerically equal to and spaced in exactly the reversed order to those which facilitate growth after the inflection point is reached. This inadequacy of the function is equally applicable as a criticism to our Equation 3 as a learning function.

Let no reader interpret the above paragraph to mean that we consider learning and growth two names for the same or similar processes. Perrin, according to Ettlinger (2), has made this suggestion; legitimately or not, it is hardly our problem here. But whatever evidence is offered to support the contention must come neither from the similarity of the curves of the two processes nor from the equations for the curves.[3]

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[3]Pearl has pointed out that Robertson has fallen into this error in assuming the identity of the growth and monomolecular autocatalytic processes. Cf. Robertson, *The Chemical Basis of Growth and Senescence,* Philadelphia, 1923.

In order to give Equation 3 a wider application and to account for the effects of whatever inherited sensorimotor substratum and modification of it bear on the specific function under examination, let us introduce a constant or parameter a, to be measured in terms of attainment, then in place of [1] we have

$$\frac{dy}{dx} = k(y—a)\ (l—y) \qquad [4]$$

and in place of [3]

$$y = \frac{le^{k(l—a)x} + ca}{c + e^{k(l—a)x}} \qquad [5]$$

This curve is written in "speed-time" coordinates (*cf.* Thurstone). To write it in "time-time" coordinates, let

t, the time required to perform a unit act $= \frac{y}{1}$

t_l, the limit measured in terms of $t = \frac{1}{l}$

$$a = \frac{1}{b}$$
$$k_1 = k(l—a)$$
$$c = e^{—k_1 r}$$

Making the substitutions

$$e^{k_1 x + k_1 r} = \frac{\frac{1}{t} — \frac{1}{b}}{\frac{1}{t_l} — \frac{1}{t}}$$

or solving for t,

$$t = \frac{t_e b(1 + e^{k_1 x + k_1 r})}{b e^{k_1 x + k_1 r} + t_l} \qquad [6]$$

The striking similarity between this curve and Meyer's (5) suggests that the arc cotangent function must be an approximation of the more fundamental and logically derived Equation 6.

The arc cotangent function may be written

$$t^1 = t_l + K \text{ arc cot } (sx^1 + p) \qquad [7]$$

From [6] when $x = —\infty$, $t = b$

From [7] when $x^1 = —\infty$, $t^1 = t_l + K\pi$

But if the curves are to approximate each other t must equal t^1 so

$$t = t_l + \frac{b—t_l}{\pi} \text{ arc cot } (sx^1 + p)$$

or $$sx^1+p = \cot \frac{\pi(t-t_l)}{b-t_l} \qquad [8]$$

From [6] $$kx + kr - \log \frac{t_l}{b} = \log\ (b-t) \ -\ \log\ (t-t_l)$$

and if $$t = \frac{b + t_l}{2}$$

then $$x = -r + \frac{1}{k} \log \frac{t_l}{b}$$

and from [8] $$x^1 = -\frac{p}{s}$$

If $\frac{p}{s} = r - \frac{1}{k} \log \frac{t_l}{b}$ both curves will pass through the same point.

By substracting this latter quantity from the x and x^1 variables and evaluating s we have two S-shaped curves having the same limits and both passing through the same point midway between the limits and approximately tangent to each other at this point. By substracting one curve from the other and finding the maximum difference in the usual way, we have,

$$(t-t_l)_{max} = t_l + \frac{b-t_l}{\pi} \operatorname{arc\,cot}\left(4 - \frac{b+t_l e^4}{1+e^4}\right)$$

or when evaluated

$$= 0.05b - 0.05t_l$$

approx.

The quantity on the right expressing the approximate maximum difference between the two curves when the values of the constants are properly adjusted is undoubtedly less than the experimental error.

We may conclude, then, that the arc cot function is a close approximation of the "growth" curve derived from the assumption that the rate of learning is proportional to what has already been learned and to that which remains to be learned.

Although the constants of Equations 3, 5, and 6 can be evaluated, the process is long and tedious and since the parameters in these curves do not differ in interpretation from those of the simpler Equation 7, it seems a needless refinement to engage in their calculation. We would, therefore, suggest the use of the approximation in fitting learning records.

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TREMOR FREQUENCIES

Lee Edward Travis and Theodore A. Hunter

In recording tremors for the past several years with a Phonelescope (2), the senior author has noted that the tremors presented at least three distinct frequencies. The most commonly recorded one of these was from 8 to 12 tremors per second. One of the other two frequencies recorded was of from 40 to 50 tremors per second and the other of from 100 to 150 tremors per second. In most instances one tremor frequency was superimposed upon another, and the higher the frequency the smaller the extent of the tremors. It was felt that the phonelescopic technique was not sufficiently refined to clearly show the tremors of higher frequency and smaller intensity. The present paper reports an attempt to refine the method for recording tremors together with some results obtained by the improved method.

By means of a hot-wire sphygmograph and a string galvanometer, Hill (1) in 1921 was able to show that a tremor frequency of around 50 tremors per second could be recorded in addition to the usual tremor frequency of 8 to 12 tremors per second. He found that the higher frequency was superimposed upon the lower with tremors of much smaller excursion. To our knowledge no one has followed up this lead.

The Apparatus

We have constructed an electrical current generator to activate a supersensitive element in our Westinghouse oscillograph. The supersensitive element has a current sensitivity of approximately .02 amperes per inch deflection.

Figure 1 shows a cross-section through the circular magnetic structure of the generator. The field coil, *E*, is a 26,000-turn coil composed of No. 31

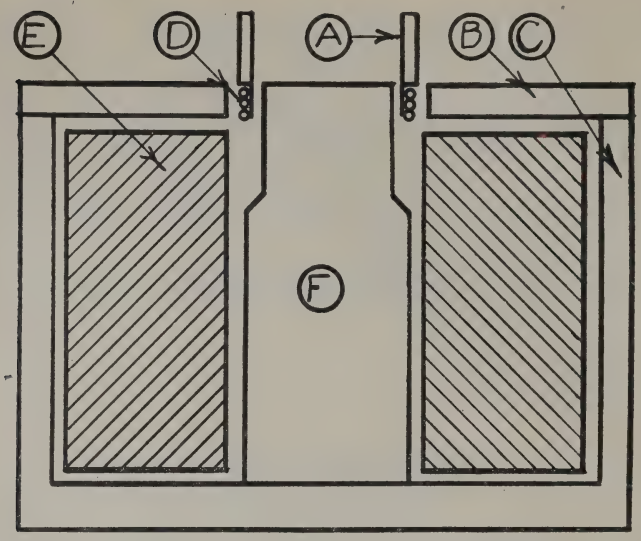

FIGURE 1

CROSS-SECTION OF GENERATOR
Description in text.

B. and S. Gauge Enamel wire. The iron magnetic circuit, *BCF,* is made of Magnelmetal, a special maluable casting produced by the National Maluable and Steel Casting Company of Indianapolis. This material is very efficient from the magnetic standpoint. The smallest section of the pole piece, *F,* is 1″ in diameter. The Formica coil form, *A,* carries a 63-turn winding, *D,* of No. 29 SS B. and S. Gauge wire. This form is mounted in such a way that it moves freely in the .077-inch gap between the pole piece, *F,* and the cover, *B.* For good efficiency the coil, *E,* should carry at least 45 milliamperes of current. All parts of the shell should be at least 5/6″ thick. The total flux in the gap between *F* and *B* is approximately 60,000 lines by measurement with a flux meter. For a given velocity of the coil form, *A,* the output voltage of this generator may be calculated from the data given by means of the equation

$$E=HLVH\ 10^{-8}$$

where E=volts, the first H=the number of wires cutting the field, the second H=the strength of the magnetic field (the number of lines of force per square centimeter section of the field), L=the length of the conductor in centimeters, and V=the velocity of the conductor in centimeters per second. This equation is derived from the equation

$$E=\frac{D\phi}{DT}10^{-8}\text{ volts}$$

As a source of direct current supply we are finding a B-battery eliminator, which furnishes 180 volts, very satisfactory.

Figure 2 is a photograph of the generator. *A* is the top of the Formica coil form, *A*. The part of the body from which one wishes to obtain the tremors is placed lightly on this top. The coil form is suspended by means of four narrow strips of dental rubber dam drawn tightly. *B* is the output wires of the generator, and *C* is the wires carrying the 180 volts from the B-battery eliminator to the field coil of the generator.

Tremor Frequencies

Figures 3, 4, and 5 show tremors recorded by means of the generator and the oscillograph. The tremors were obtained from the extended fingers of the right hand while the arm was firmly supported at the elbow.

In terms of muscle balance, tremors are composed of two phases, the extensor and the flexor. For the large tremors the extensor phase is indi-

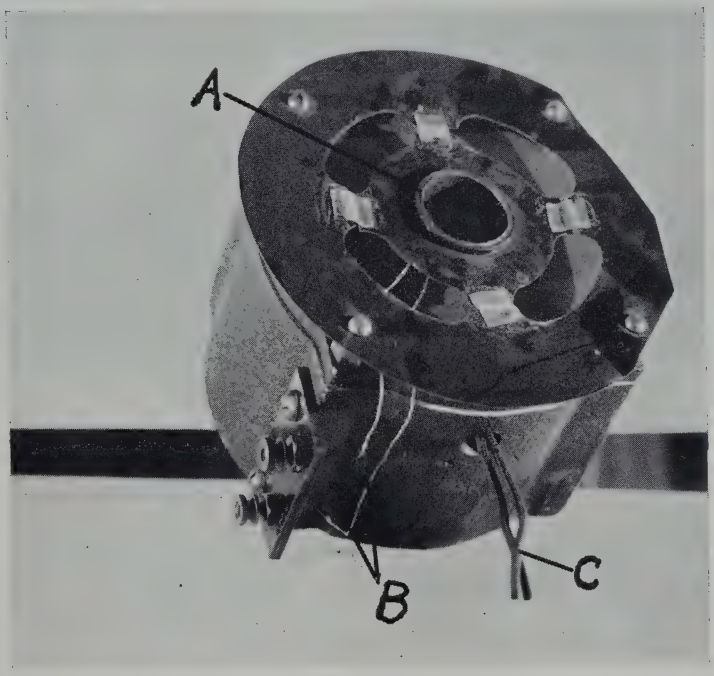

FIGURE 2

Photograph of Generator

Description in text.

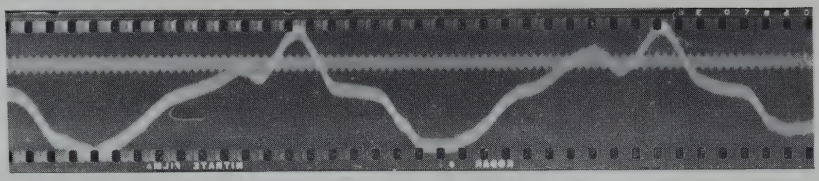

FIGURE 3

RECORD OF TREMORS FROM THE EXTENDED FINGERS OF THE RIGHT HAND

In this and in all subsequent records an upward sweep of the tremor line indicates an increased extension of the fingers and a downward sweep a decreased extension of the fingers. Time is represented in 1/500 seconds.

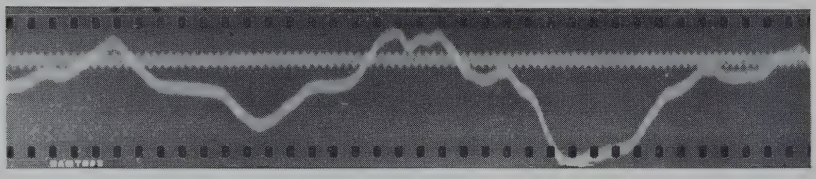

FIGURE 4

RECORD OF TREMORS FROM THE EXTENDED FINGERS OF THE RIGHT HAND

Time is represented in 1/500 seconds.

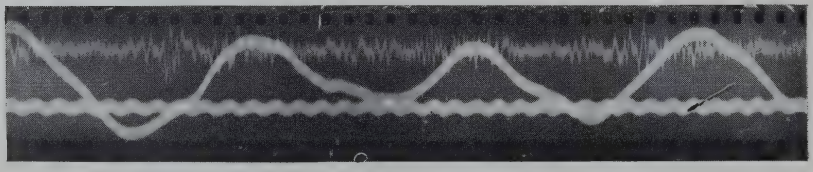

FIGURE 5

RECORD OF TREMORS FROM THE EXTENDED FINGERS OF THE RIGHT HAND AND OF ACTION CURRENTS FROM THE EXTENSORS OF THE WRIST OF THE RIGHT ARM

Time is represented in 1/120 seconds.

cated by an upward movement and the flexor phase by a downward movement in the tremor record. For the smaller tremors this is not always the case. If a smaller tremor appears on the upward or extensor phase of a larger tremor the general direction of movement of both of its phases may be upward. The opposite may be true if it appears in the downward phase of a larger tremor. In these instances the smaller tremor appears as a small hump on an otherwise relatively straight line. In Figure 3 tremors of a rate of 40 per second are superimposed upon tremors of a rate of 10

per second. This is a very common occurrence. In Figure 4 there is one large tremor occupying one-tenth of a second with two other tremor frequencies superimposed upon it; one of 40 tremors per second, and the other of from 100 to 150 tremors per second.

Figure 5 shows the action currents from the largest portion of the extensors of the wrist (brachioradialis and extensor carpi radialis longus) recorded simultaneously with the tremors. Our method for recording action currents has been described elsewhere (4). Inasmuch as the electrodes were placed on the extensor muscles of the wrist, we recorded action currents mainly during the extension phases of the tremors. The correspondence between the increase and extent of the action currents and the upward movement of the large tremors is clearly indicated in this record. It is to be noted that the beginning of the increase in extent of the action currents precedes the beginning of the upward phase of the large tremors by approximately 1/60 of a second. Neither the increase nor the decrease in the size of the action currents is perfectly regular by any means. There appear smaller groupings of action currents within the larger groupings. It is to be noted further that there is a fairly close correspondence between the smaller groupings of the action currents and the extensor phases of the smaller and more rapid tremors. The interval of time elapsing between the beginning of the increase in size of the action currents and the beginning of the extensor phase of the tremors is undoubtedly dependent somewhat at least upon the length of the arm, hand, and fingers.

The action current picture may best be envisaged as a modulated wave, that is, a wave having the amplitude of its oscillations varied periodically. This modulated wave is made up mainly of an audible or principal frequency and an inaudible frequency. The inaudible frequency, 8 to 12 per second, is composed of the periodic variations in intensity of the audible frequency, 300 to 600 per second, which we have previously termed the "envelope" (3). This envelope shows the nature of the electrical change in the audible frequency. It is quite evident, as we have noted previously, that the envelopes of a frequency of 8 to 12 per second are made up of smaller envelopes to correspond roughly in frequency with the tremors of 40 to 50 per second. From such records it seems reasonable to suppose that there are three or more relaying centers in the central nervous system, one furnishing the principal audio frequency, another the modulating inaudible frequency which causes the larger and slower envelopes and the third the audible frequency which causes the smaller and faster envelopes. The two envelope frequencies have their counterparts in tremor rates, 8 to 12 and 40 to 50 per second. The former study (3) indicated that the principal audio action current frequency (300 to 600 per second) had its counterpart also in a tremor frequency. With our present apparatus for recording tremors we have not recorded a tremor frequency above 200 tremors per second.

We are carrying forward studies to settle the following points: (*a*) the effect of various psychological and neurological states upon tremor frequencies and intensities; (*b*) the factors involved in determining the length of time elapsing between the beginning of increase and decrease in extent of the action currents and the extensor and flexor phases respectively of the tremors; and (*c*) the factors responsible for the relation of one tremor frequency to another.

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FIRST INTERNATIONAL CONGRESS OF RELIGIOUS PSYCHOLOGY

The First International Congress of Religious Psychology will be held at the University of Vienna from May 26th to May 31st (Week of Whitsuntide), 1931, under the auspices of the International Society for Religious Psychology, the President of which is Karl Beth, D.D., Ph.D., Professor of Protestant Theology in the University of Vienna, and the Vice-Presidents Franz Brandl, LL.D., Director of the Viennese Police, and Otto Nahrhaft, LL.D., First Public Prosecutor.

The most important problems of recent religious psychology will be discussed from various points of view, educational, experimental, sociological, psychiatric, pathological, theological, occultistic, etc., the main subject being the psychic basis of the religious unbelief of the present day.

Many internationally known scientists are scheduled to deliver lectures or read papers at this Congress.

Opportunities to see the city of Vienna and its important institutions will be amply provided for.

Inquiries as to program, facilities, schedule of sight-seeing trips, and all other information should be sent to the International Society for Religious Psychology, Vienna, VII. Zitterhofergasse 8, Austria, and should be accompanied by fifty cents in stamps.

ERRATUM

Three incorrect numerical values were given in the footnote on page 32 of the article by Ferree and Rand entitled "Size of Stimulus and Brightness of Preexposure and of Surrounding Field in Relation to the Extent and Shape of the Color Fields" which appeared in *The Journal of General Psychology,* 1930, Volume 4. In line 6 of this footnote, 0.00001 mm. should read 0.0045 mm.; in line 7, 0.0000067 mm. should read 0.003 mm.; in line 8, 0.000005 mm. should read 0.00225 mm.

APPARATUS

AN ELECTRO-CHEMICAL PARALLEL TO THE CONDITIONED REFLEX

ROBERT G. KRUEGER AND CLARK L. HULL[1]

The past few months have witnessed the appearance of three distinct models designed to duplicate one phase or another of adaptive mammalian behavior (8, 14, 15, 1).[2] A fourth investigator is approaching the problem from the point of view of mathematical physics (12). Apparently each of these projects was carried out entirely in ignorance of the others. While it is true that this is not the first time that a model intended to parallel adaptive behavior has been designed (13, 10), the practically simultaneous appearance of four or five independent efforts in this direction is not without significance as to the trend of the times. The present article contributes the description of still another model to this unique literature.

It has been pointed out by Baernstein and Hull (1) that a polarizable cell manifests behavior strikingly similar to certain aspects of the conditioned reflex. The present writers, utilizing this fact, have combined several such cells in a simple manner to form a purely electro-chemical model paralleling in still greater detail the behavior of the conditioned reflex. The wiring diagram of the model is shown in Figure 1. The circles at E represent two dry cells. They are connected in series and furnish a potential of 3 volts. L, the responding mechanism, is a 2½-volt flashlight bulb connected in series with the battery, E. S_u, which represents the unconditioned stimulus, is a single-pole push button which completes the unconditioned stimulus circuit. The circuits containing the neutral stimuli to be conditioned consist of the radio "B" storage cells, E_1, E_2, E_3, E_4, and E_5 connected in parallel with the light. S_1, S_2, S_3, S_4, and S_5 are single-pole push buttons placed in series with the corresponding storage cells and connecting each with the responding bulb. Resistances of from 80 to 120 ohms, R_1, R_2, R_3, and R_4, connect the positive plate of each cell to the positive plate of the adjacent cells.

THE UNCONDITIONED REACTION

If S_u alone is pressed, current from battery E will pass through the filament of L causing it to become luminous. This represents the uncondi-

[1]The model was designed and constructed by the first author. The basic psychological ideas and the formulation of the results are due to the second.

[2]A fifth independent attempt has come to our attention through personal correspondence.

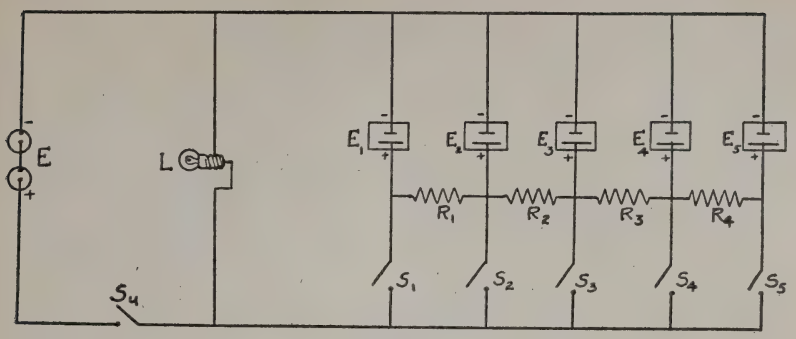

FIGURE 1

WIRING DIAGRAM OF ELECTRO-CHEMICAL MODEL PARALLELING MUCH OF THE BEHAVIOR OF THE CONDITIONED REFLEX

tioned reaction. But if S_1, S_2, S_3, S_4, or S_5 is pressed, no reaction will follow because the corresponding storage cells are as yet uncharged (11, p. 25).

THE PROCESS OF CONDITIONING

If S_u and any one of the neutral push buttons such as S_2 be pressed simultaneously for a fraction of a second, the bulb will glow as in the ordinary unconditioned reaction. If, now, S_2 be pressed by itself, a faint glow may be seen on the filament. This means that S_2 has acquired the capacity to evoke the response formerly evoked by S_u. Thus the model parallels the process of conditioning (11, pp. 36 ff.; 2, p. 341). If this conditioning process is performed repeatedly and a brief test is made after each repetition, it will be seen that the glow of the filament becomes progressively brighter until a maximal reaction is reached. If successive photometric determinations of its brightness should be made and the results plotted in the conventional manner, presumably a very creditable "curve of learning" would be obtained.

The explanation of this conditioning reaction is that when S_2 is in contact, part of the current otherwise flowing through L now flows through S_2 and its storage cell, E_2, which charges the latter. When S_2 alone is pressed the charge thus acquired by E_2 now flows through L and produces light. Thus the learning process in this model consists in an accumulation of energy at strategic points, rather than a synaptic change at strategic points capable of releasing energy from a general reservoir, as is usually assumed in neurological accounts of learning.

SECONDARY CONDITIONING

If S_2 has not been much over-conditioned a test of the remaining push buttons will evoke no observable light reaction. Now let S_1 be pressed

simultaneously with S_2 for a short time. S_2 will, of course, evoke a light reaction. But if, later, S_1 be pressed alone, it will also cause the filament to glow, though the reaction may be somewhat weaker than that from S_2. And if S_1 be pressed simultaneously a short time in conjunction with S_4, say, and later S_4 be pressed alone, the latter may also have acquired the capacity to produce a glow in the filament. In this case the glow from S_4 will pretty certainly be very weak. If the attempt is made to transfer the reaction from S_4 to S_3, say, it will probably fail.

These results parallel very closely the phenomena of secondary conditioning as observed in dogs, not only as regards the mere transferring of the reaction from any one stimulus to any other, but as to the details of the process such as the progressive weakening of the transferred reaction and the practical limit as to the number of times the process may be repeated (11, pp. 33 ff.). The phenomenon of secondary conditioning of S_1 results from a part of the current otherwise flowing from E_2 through L being diverted by the closure of S_1 so that it now flows through E_1 with the consequence that E_1 receives a charge. This charge must be less than that of E_2 because, among other things, part of the latter's charge must go through L.

Irradiation

As soon as the process of conditioning to a given stimulus begins, the storage cells corresponding to the remaining stimuli also begin to charge but at a less rapid rate. This is the basis of the irradiation phenomenon. The cells not directly involved in the conditioning process may not get sufficiently charged to bring the filament above its threshold of luminosity. But if any stimulus, such as S_1, be somewhat over-conditioned, there will come a time at which S_2 will also call forth the luminosity of L, though in a somewhat weakened degree, and even S_3 may show it if the original conditioning is sufficiently prolonged, though in a still weaker degree. This parallels rather closely the phenomenon of irradiation of conditioned reflexes as observed in dogs, particularly where skin stimuli are involved. In general, the strength of the irradiated reaction grows progressively weaker the farther away it is from the point conditioned (11, pp. 152 ff.).

The tendency to irradiation in the model results from the fact that when one neutral stimulus, such as S_1, is pressed in conjunction with S_u, the current from E tends to flow not only through S_1 to E_1 but also to E_2, E_3, and all the other cells. But since the flow of a divided current is inversely proportional to the resistance in the several alternative pathways, the current through E_2 is reduced by the resistance of R_1 and that through E_3 still more by the combined resistance of R_1 and R_2.

The Conditioning of Compound Stimuli and the Phenomenon of Redintegration

The model shows not only the conditioning of single end-organs, but the conditioning of groups or complexes of the neutral stimuli. If S_1, S_3, and S_5 are pressed a short time in conjunction with S_u, and the combination is later pressed without S_u, the filament will glow. If this compound conditioning is conducted carefully so that it is interrupted at the moment it gets just strong enough to evoke the maximal glow, a nice parallel to the phenomenon of redintegration will result. This manifests itself in the two facts (*a*) that if any one of the three stimuli is pressed it will evoke the reaction, and (*b*) that the reaction so obtained is perceptibly weaker than that evoked by the entire stimulus complex. This appears to agree fairly well with the conditioned reflex as observed in dogs provided the components of the compound stimulus are of equal strength (11, p. 56, pp. 141 ff.).

Summation

If two neutral stimuli have been independently conditioned to L_1, each so lightly that it produces a faint glow of the filament, the two, when pressed simultaneously, will produce a preceptibly brighter glow. Similar summation phenomena are observed among conditioned reflexes with dogs (11, p. 79; 2, p. 343.).

Experimental Extinction

If an end-organ or a group of them have been conditioned to the light, and this conditioned reaction is evoked repeatedly by applying the conditioned stimulus without reinforcement, the reaction will gradually weaken and finally disappear. This parallels rather closely the corresponding behavior of true conditioned reflexes (11, p. 49). If the original conditioning has been slight, the reaction will not survive many unreinforced stimulations, whereas if the conditioning has been thorough, the conditioned tendency will be quite stable, persisting through many unreinforced stimulations. This phenomenon in the model results from the gradual discharge of the storage cell or cells involved in the conditioning tendency.

Spontaneous Recovery

If the process of experimental extinction has been fairly rapid, and if, following this, the model is left undisturbed for a time and then tested, it will be found that the conditioned reaction is again present, though probably in a weakened state. Repeated use may again extinguish the reaction—this time more quickly—after which a period of "rest" will be followed by a second recovery, this time still less complete. In this the model parallels in some detail the spontaneous recovery of conditioned reflexes (11, p. 58). This phenomenon in the model results from the fact that the ordinary

storage cell will not deliver its full charge at once, but requires a certain amount of time to complete the process.

Differentiation

If several stimuli have acquired the capacity to evoke the light reaction, either through irradiation or compound stimulation, it is possible to single out any one of these for survival and to extinguish the others. The mode of doing this parallels very closely that employed in a similar procedure in the development of the mammalian conditioned reflex (11, p. 417). Alternately the several end-organs are stimulated, the one destined for survival being consistently reinforced by association with S_u, whereas the others are not. In a short time the one will evoke the light maximally and the others will evoke no response. This result is brought about in the model, just as it is in the mammalian organism, by a combination of the processes producing reinforcement and experimental extinction.

Secondary Extinction

Just as there is a negative aspect to conditioning which appears as experimental extinction, so there is a negative aspect to irradiation. This is called secondary inhibition. Suppose two adjacent stimuli, as S_3 and S_4, are lightly conditioned, but S_4 twice as heavily as S_3 If S_4 be extinguished by stimulation without reinforcement for, say, 15 seconds beyond the point when the filament ceases to glow, it will be found that S_3 has also become inhibited. If the reverse procedure be employed, it will be found that S_4 will have survived, though it may be weakened. This closely parallels the behavior of the conditioned reflex under similar circumstances (11, p. 56).

Forgetting

If left alone long enough, conditioned reflexes finally decay and cease to function. This is also the case with the present model. If left for some weeks or months, even a well-charged cell will slowly discharge itself. Thus a conditioned stimulus will ultimately, through the mere passage of time, lose its capacity to evoke the reaction and become neutral.

Summary and Conclusions

The electro-chemical model described above parallels fairly accurately the behavior of the mammalian conditioned reflex, as now understood, in a number of important respects:

1. In its original state the model presents an unconditioned receptor and a number of neutral receptors.
2. Any one of the neutral receptors, if stimulated in conjunction with the unconditioned receptor, will gradually acquire the capacity independently of evoking the reaction.

3. A second conditioning may be secured from the first conditioned reaction, and a third from the second, but there is a limit to the number of secondary conditionings possible.

4. When a conditioned reaction has been set up to a receptor, there is a tendency for it to irradiate to neighboring receptors.

5. Compound stimuli may be conditioned just as single receptors.

6. The individual receptor components of compound stimuli may each evoke the conditioned response evoked by the whole, but often in a weakened state.

7. Two or more independently acquired submaximal conditioned tendencies will combine summatively.

8. Repeated use of a conditioned reaction without reinforcement gradually weakens it until it will cease to respond.

9. Conditioned reactions thus weakened by experimental extinction, if left for a time undisturbed, will spontaneously recover.

10. Where several conditioned stimuli independently evoke the same reaction, the reaction may be limited to any selected stimulus by a process of reinforcing the one while alternately stimulating the others without reinforcement.

11. The experimental extinction of the reaction conditioned to one receptor tends to weaken a conditioned tendency to the same reaction originating in a neighboring receptor.

12. The passage of time alone will produce a decay of a conditioned reaction tendency, which parallels forgetting.

It is believed that the construction and study of models of the type described above will aid in freeing the science of complex adaptive mammalian behavior from the mysticism which ever haunts it. The belief is very widespread and persistent that certain complex forms of adaptation cannot take place by any imaginable concatenation of materials without the mediation of some *nous, entelechy*, soul, spirit, ego, mind, consciousness, or *Einsicht*. There is, on the other hand, the opposed belief that the above explanatory concepts are but the names of disembodied functions (ghosts) which, insofar as they have any objective existence, are themselves in need of explanation and may conceivably be duplicated by adroit concatenations of materials. The conflict between these points of view is probably at a permanent impasse unless the second party can bring forth tangible evidence of synthetic achievement in this direction. That a considerable degree of success in this practically virgin field will reward a vigorous effort can scarcely be doubted. As progress is made by the second group it may be anticipated that the first will gradually retreat to the more and more inaccessible parts of the psychological terrain.

How successful the attempts to construct "psychic" machines will prove, time alone can tell. It will be too early to venture a final judgment until

as much labor and ingenuity have been expended in the attempt as have been devoted to the development of the steam engine, the electric motor, or the printing processes. Unless some practical use for such adaptive mechanisms should develop, this will naturally be a very long time. It is not inconceivable, however, that in the demands for higher and higher degree of automaticity in machines constantly being made by modern industry, the ultra automaticity of the type of mechanism here considered, may have an important place. In that event the exploration of the potentialities lying in this radically new order of automaticity would be comparatively rapid.

It is possible, even now, to foresee some of the problems which must be solved before a mechanism of a really high order of spontaneity and intelligence can be evolved. First may be mentioned the problem of designing a mechanism such that reaction may be conditioned to certain combinations or patterns of stimulated receptors as distinguished from the simple summation of excitatory or inhibitory tendencies of the individual stimulated end-organs (4). A second task, and possibly the most difficult of all, is the general solution of the mechanical problem of how to enable a given combination or pattern of stimuli conditioned through a particular set of receptors to function with little or no loss in power through a pattern of end-organs, which is similar or equivalent, but which may have never before been themselves involved in this reaction (9, pp. 138ff.). A third necessity of the greatest importance is the discovery of a design which will provide for the ready evolution of pure stimulus acts (individual organic symbols) which appear to be mainly the basis of the ability of the mammalian organism to react to situations at a distance or in the future (6). A fourth major problem is that of somehow bringing forward to the beginning of purposive action sequences, so as to serve as a special component of the part of the stimulus complex persisting throughout the behavior series, some part or equivalent of the goal-reaction terminating sequences of purposive behavior (7). Other obvious necessities, some of which are involved in more or less complex ways with those previously listed are: a provision for conditioned inhibition (11, pp. 68 ff.), for the evolution of remote excitatory tendencies (3), for the automatic stimulation of resident end-organs by each act of the mechanism, for an assortment of dynamic persistent stimuli (drives) (5), and for the easy evolution of families of habits, all the habits of a given family being equivalent in the sense that, while the preliminary acts of each may be totally different, the action sequences will have the same goal (7).

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UNIDIRECTIONAL MAZES FOR STUDYING HUMAN LEARNING

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Through recent experimentation and discussion, the maze as an instrument in the psychological laboratory has been developed into several fundamental types, which, all told, present quite a range of technological possibilities. Both usual and unusual types may be classified into five main groups on the basis of the material and the problem. (*a*) *Perceptive material.* The objective versus the abstract maze. The former is, of course, the usual sort used in human and animal learning. The latter is typified by the mental mazes of Peterson (14), which need have no objective or even diagrammatic existence. (*b*) *Experiential mode.* Spacial versus temporal mazes. The temporal patterns as developed and employed by Hunter (5), of course, make use of objective space mazes, but in the particular way that some part is used more than once in succession. Learning a

problem of this kind demands that the subject retain a portion of the experience as representative. (*c*) *Sensory characteristics specifically of the path.* Enclosed, tunnel (15), or grooved maze versus open or elevated maze. Among the instruments under this head comes the high-relief maze proposed by the writer for use in the study of human learning. This type makes for prompt, rapid learning, since, in addition to kinaesthetic experience, there is direct cutaneous experience from the contact of the finger with the path.[1] A narrow raised path tends to reveal to the subject all of the joints or openings as he comes to them. (*d*) *Sensory modality of the maze as a whole.* Blind maze versus visible maze. The typical maze for human subjects has required the rather inconvenient and unnatural situation of being blindfolded. Carr (2) and Perkins (13) have described mazes for use while under visual inspection and Miles has employed a two-story maze, which provides the maximum of indirect visual guidance. Now we have available the Barker stepping-stone maze (1), which, like the Carr maze, is fully visible. However, it is more simple to construct, and offers within the same instrument a great range of possible maze problems. The Barker visible maze furthermore has the same versatility for framing problems that is shown by the Chou (3) high-relief blind maze. (*e*) *Directional aspect.* The bidimensional versus tridimensional maze. This distinction has been recently made by Hunter (6, 7), who has shown the usefulness of the tridimensional maze, particularly in connection with his interesting problem on the variations of the temporal pattern. Hunter's work has been with animals, but tridimensional mazes are already beginning to appear for use with human subjects.[2]

The purpose of this note is to describe and to discuss briefly some mazes that have been in use in the Stanford Laboratory for about two years, and which, I think, offer an important deviation from certain of the other types that have been mentioned. For want of a better name, we may call these mazes unidirectional. Of course, in the name "unidirectional maze" we have really a clash of terms; the word "maze" seems necessarily to indicate more than one direction or dimension, whereas the term "unidirectional" would seem to be shorn of all deviations from a straight line. The essential feature of the unidirectional maze is that the successive turns, to the right or left, do not serve to carry the hand of the subject away from the median line along which the maze is laid out. Each correct

[1]The narrow elevated open maze for rats (9, 11) also shows more prompt learning than is found for alley mazes of the same ground plan, but probably because the open maze offers less space area to explore and gives the rat more useful visual cues.

[2]Mazes can be classified also in reference to simplicity and uniformity versus complication and variety in blinds, in reference to opportunity for or obstruction to retracing and other features. Probably these hardly deserve being designated as fundamental types.

choice or turn provides its own immediate return to the central linear axis of the design. Thus, by slightly devious movements, the subject progresses to the goal in a single general direction.

When a human subject sits down in front of a maze, or has one placed in front of him, without explanation on the part of the experimenter, he knows or naturally may assume that the pattern is spread out in two directions, and "of course within reach." If in tracing a piece of true path, or several successive pieces, his arm is carried far to the right, relative to his posture and what he conceives to be the general dimensions of the pattern, he will then exhibit a tendency to take turns to the left. This strong tendency was particularly well shown in the work of Husband (4). He used the Warden U-pattern (16), made up in the form of a high-relief finger maze. His complete maze contained three sets of triple sequences, that is, where three successive turns of true path were to the right, or to the left. If we let the total number of errors made by entering the blind opposite the first turn of these sequences represent 100 per cent, then we find that the errors from entering the blind opposite the second turn equal 136 per cent, and those for the blind opposite the third turn in the succession equal 292 per cent. The unidirectional maze as illustrated in Figure 1 is practically free from such a difficulty.

It may be argued that the subject traversing this maze will realize through counting that he has had, let us say, three turns to the right, and he may guess that the next turn will be to the left. However, in this type of maze the subject is put on his guard against making these assumptions. He realizes at once that there is no structural reason why all the choices might not be on one side, and, since the arm is never carried far to the right or to the left, the predisposition for guessing in one direction is much reduced.

In the bidirectional and tridirectional mazes as ordinarily employed there are really two things to learn: first, the general layout of the pattern; this may be approximately circular, elliptical, spiral, zig-zag, etc.; the second task is to learn the succession of turns. The unidirectional maze is, of course, practically free from the first-mentioned factor. The pattern is all in one line; therefore the only problem is to learn the correct succession of turns. In this characteristic the unidirectional maze is very similar to the abstract maze.

Since with the unidirectional maze there is nothing to hide so far as general diagram or scheme of layout is concerned, it is quite feasible, and in fact seems to me desirable, to show the subject the general pattern of the maze when introducing the experiment to him. Proceeding in this manner, the introduction and directions which we have given our subjects when conducting experiments with these mazes have been approximately as follows.

> "In this experiment you are going to learn a simple series of turns which you will trace over with the hand while wearing a blindfold. This diagram (showing a full-size tracing on a cardboard) is a schematic representation of the entire pattern and shows you the unitary form of its construction. You see that it is laid out in four rows, which are connected by rather long links. These are easy to identify when you come to them. You start at the lower left-hand corner, tracing your way up to the top, shift over to the right, trace downward, and so on. Notice that the path is made up of squares placed corner to corner. You will trace along the sides of these squares, beginning at the place marked 'start' on the diagram. You trace up the right side or the left side. When you are actually doing the experiment, you will find one side blocked off around the corner. A staple has been driven over the path, and will obstruct your finger in its progress. The block is always placed around the corner. Before you have tried it, you cannot possibly tell which side is blocked off. One side is about as likely to be blocked off as the other, so at first your success will be largely chance; you will have to remember, so as to succeed well in the next trial. If you happen to turn into a blocked path you will simply go back and trace up the other side of that square."

Two mazes are illustrated in Figure 1; *A* is made in high-relief form, while *B* shows exactly the same succession of turns made in the form of a stylus maze. In *A* the individual units or squares are one inch on each side, and each side is made by driving a single wire staple into the panel. German silver wire, size No. 14 (1/16" diameter), is used in this construction, as in the other high-relief mazes which we have made (10). The long lengths connecting strips of the linear path are also of this wire. These lengths are inserted so that the sections of linear arrangement may be conveniently within reach of the subject. Each maze is mounted on a three-ply panel, 18 inches square.

The construction of the stylus maze, *B* is accomplished very simply by the method described by Knotts and Miles (8). To the three-ply panel is fastened a sheet of tin that covers the whole of one side. The plan of the maze is laid out accurately on coordinate paper, and this is fastened to the sheet of tin. Then small blocks of appropriate size, cut from three-ply semihard wood, are nailed over the diagram and through the tin into the panel. In this manner the border of the path is filled in accurately and with square corners. Finally the paper is cut away from the pathway, leaving a smooth bottom for the stylus to glide on. The grooves in the present maze are 3/16 inch wide. The small wood blocks which make up the centers of the grooved squares are 13/16 inch square. In the case of the stylus maze staples are driven across the path, as they are driven over the wire path in the high-relief maze. A stylus that fits fairly well the 3/16-inch width of the groove helps the subject to find all the openings, but

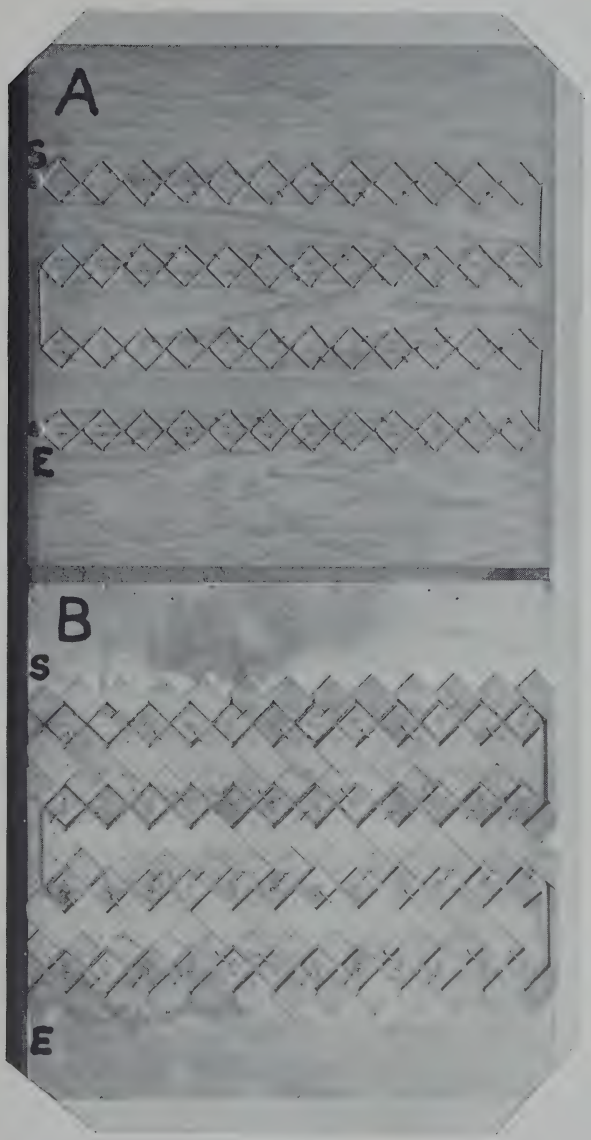

FIGURE 1

UNIDIRECTIONAL MAZES ARRANGED COMPACTLY AS PARALLEL SECTIONS OF PATH

A, high relief form made of wire to be traced by finger contact. *B*, stylus form. *S*, start. *E*, end. If the entire maze of 48 choices is too long, 24 or 12 may be used conveniently.

this is always a difficulty particularly with stylus mazes. The stylus unidirectional maze proves to be much harder to learn than the high-relief form. This is confirmatory of Nyswander's (12) results.

It is, of course, quite obvious that the succession of turns may be readily changed in such mazes by simply lifting the staples and fastening them in other places. In fact a pair of holes can be made on each side of each unit all ready for the quick transfer of the staples from one position to another.[3]

The unidirectional maze has not been described for the sake of filling out a logical scheme by the addition of another type of maze instrument. To the writer's mind it represents a real step in the methodology of maze experimentation. Other experimenters have doubtless taken this same step, and quite probably through the arrangement of similar linear mazes. The point which I wish to make has long been recognized in most of our experiments in the psychological laboratory. It has to do with the presentation of a standard and uniform problem so limited as to material and procedure that one or more specific aspects of behavior can be thereby studied or measured. We instruct a subject about to undertake an experiment in order to present a definite objective, so that he will understand what he is to do as clearly as possible. In the maze experiment, however, we have literally allowed the subjects to find their own way. Thus the problem has been complicated by a number of factors, such as (*a*) chance error; (*b*) the double situation to learn, that is, the general layout and the succession of turns; (*c*) variety in the kinds or lengths of blinds within the same maze; (*d*) openings that lead off as T's and L's and X's; and so on. With such a complex situation we cannot expect a very high reliability, or much uniformity in subjective methods of attack.

In summary, it may be stated that the unidirectional maze as an instrument tends to define more precisely the learning task of the subject. (*a*) This maze in diagram (or the object itself) without the positions of the blinds being indicated can be shown to the subject. This clarifies the problem, as well as reducing the amount of time required for adequate instruction. (*b*) The unidirectional maze lends itself well to the counting method; in fact it directly suggests to the subject this method of attack, and so causes more of the subjects to try to do the same thing. (*c*) Retracing is automatically eliminated, by reason of the unitary character of the construction. (*d*) The succession of turns may be any desired combination, without making an awkward shape difficult to administer, and without accentuating the tendency for alternation. (*e*) This style of maze permits great compactness, and inadvertent exposure to a subject is not a calamity.

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[3]A temporary or trial form of this maze can be arranged by tacking several square wooden blocks in line, corner to corner, and using thumb tacks in the edges for the stops. With tack holes on both edges of each block the maze itself may be shown to the subject and the tacks inserted after the blindfold is put on.

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UNIDIRECTIONAL MAZES FOR USE WITH ANIMALS

Walter R. Miles

Human subjects can probably learn more rapidly in a unidirectional maze than in the ordinary bidirectional pattern.[1] This is for the reason that we have not as a rule given the subjects any instruction as to the general layout of the paths. We have expected them to learn this feature along with that of the succession of turns. When we come to animal learning, we find that

[1]For a discussion of fundamental maze types see the previous note (1).

the unidirectional maze is more difficult for the rat than is the bidirectional. This is our experience, and it seems to be indicated on theoretical grounds since the linear maze presents a rather less varied array of spatial cues for the use of the animal than does the pattern that is spread out as a series of T's or L's. Therefore, for controlling and contrasting different arrangements of spatial cues or elements, it is desirable to have a maze that can be arranged in as linear a fashion as possible. *A,* in Figure 1, shows such a maze in the form of X's or square boxes, arranged corner to corner. In this maze the rat is entered at the left, and goes around the enclosed box, which is found in each unit. The path around the box on one side is closed by a screen at its farther end, so that the animal must at least go to the first corner before he can discover whether or not the path is open. Each choice, if correct, brings the animal back to the median line. When

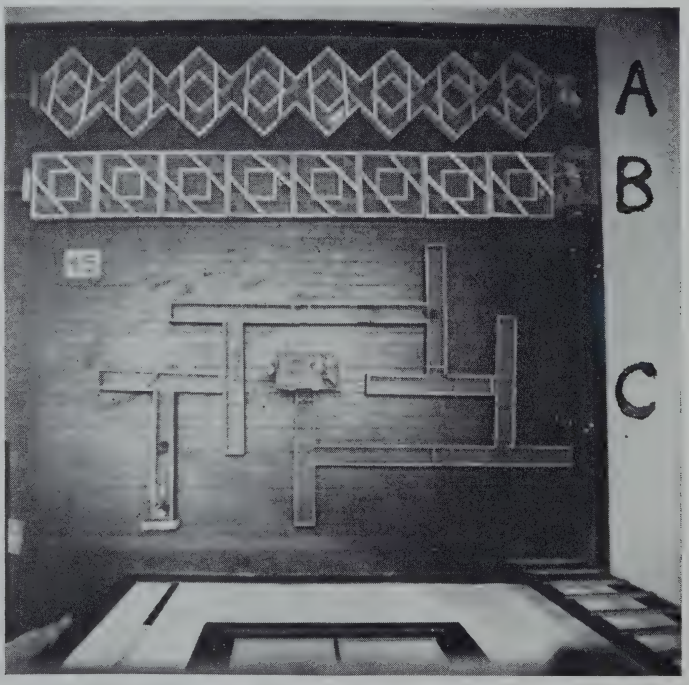

FIGURE 1

UNIDIRECTIONAL (A AND B) AND BIDIRECTIONAL (C) MAZES FOR USE WITH RATS

All three are sectional in construction and as set up have the same series of turns

the blinds alternate left and right in successive units, the pieces of true path connecting from one unit to the next make a straight section in which the animal is properly oriented to go correctly. If the blinds do not alternate, then the animal at certain places is led directly into a blind. This X-form therefore predisposes to the entrance of some blinds considerably more frequently than to others. The difficulty may be partly overcome by placing small intermediate doors or collars to be gone through before entering each next unit.

Square units placed side to side as in *B* of Figure 1 exhibit much less of the tendency for leading animals into blinds. With the square form of unit, a blind, although it must be followed practically to the end before it can be discovered as a blind or a piece of true path, is not quite as long as the corresponding piece of true path. Results on these two unidirectional mazes will be published elsewhere. The X-form in its true path resembles pretty closely the symmetrical linear maze of Warden (2), except that the blinds all lead in, whereas those in the Warden maze lead to the periphery. Our square units closely resemble those of Liggett (3), but are smaller and less complicated.

The units in both of our unidirectional mazes are of the same size; the walls are made of ¾-inch wood, 4 inches high, the alleys and blinds are 4 inches wide and covered with half-inch mesh galvanized wire. The inside square, around which the animal must go, measures 8½ by 8½ inches. The over-all size of each unit is 18 x 18 inches. Braces across the outside frame and the inside square stiffen the unit and make it unnecessary to have a bottom in it. This greatly facilitates cleaning, and occasionally the retrieving of animals that have not completed the maze, or that have retraced after having reached the food. Neither maze, as I have used it, is provided with doors or stops to block retracing.

In the case of the X-maze the entrance from one section to another is 4 inches wide, viewed as alley width, and 6½ inches wide viewed as the diagonal width of the adjoining space. For the square-maze a 2-inch square hole or notch is made in two opposite sides of each unit. In the successive units these notches are placed in register so that the rat, passing from one unit to the next, goes through this 2 x 2 opening, which is 1½ inches thick, or long.

The three mazes exhibited in Figure 1 all present the same succession of left-right turns. Starting at the left of the room, the floor plan of which is shown in the photograph, the correct turns are right, left, right, right, left, right, right, right. These three mazes each presenting eight choices have been used for comparison studies. The animals have started their learning with what may be called the group method. Six rats are placed in the first unit of each maze, and a block is set against the opening to keep them in. The animals are free to explore and are left in for one hour on two or three

consecutive days. One rat learns only one maze. The mazes are shifted in their positions in the room each day. When it is observed that rats, on being admitted to the maze, are fairly promptly going through to the food cages at the right then the group method is stopped and individual runs are begun. This method seems justifiable when the main object is to compare different maze instruments and transfer to other forms or patterns on which all the animals are to be tried.

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A MIDWAY MAZE FOR WORK WITH ANIMALS

Walter R. Miles

In some recent work with ferrets, it occurred to the writer to provide an enclosure which would permit the animals to go directly to food, or, if the exploratory tendency was exceedingly great, would permit opportunity of their passing through a number of compartments which led off the main thoroughfare. It seemed that the ferret, since he is carnivorous and hunts live food, might prefer to take advantage of all possible openings, rather than to go straight ahead on a fairly clear path. It was for the purposes of such experiments that the device in Figure 1 was arranged.

The central straight section of the midway maze has a total length of 14 feet. The path is 8 inches wide, inside measurement, and 8 inches high, and all the boards are ¾-inch in thickness. At the left-hand end, as shown in the illustration, a section 17 inches long forms a starting box. This is covered over by a board on which numbers are placed for designating the experiments when making motion-picture records, and a swing door for entering the animal is placed at the end. The animal is inserted through the swinging door, D. When the experimenter is ready to start the test he raises the sliding door, D^1, by a cord attached to the post, P. The straight alley presented to the animal is 12½ feet long. A second sliding door, D^2, placed at the food or terminal end can be lowered quietly, by use of a cord also attached to P, when the animal has entered the food cage.

Openings 4 inches high and 8 inches wide have been arranged in the midway or straight section so as to join with eight maze units as seen in

Figure 1. Each such maze unit consists of a piece of path which can be entered and passed around in either direction. There is no screened partition serving as a stop. With the exception of number 5 as described here, all the units are interchangeable. The arrangement of the eight units provides opportunity for the alternation tendency and also for confusion from having two sets of two units directly opposite one another.

In Figure 1 two food cages are shown in position. Of course, both would hardly be used at once. When the food is at the end, blind 5 is closed like the other blinds, and the problem is, of course, a simple one, or so it proved to be both for the ferret and for the rat. Moving the food to position 5, which can be passed through from the midway to the food cage, and keeping the door at the end of the midway closed, we have essentially a multiple choice apparatus. The animal is allowed to explore until he finds the food. The problem is, how quickly will he get over the interference by going to

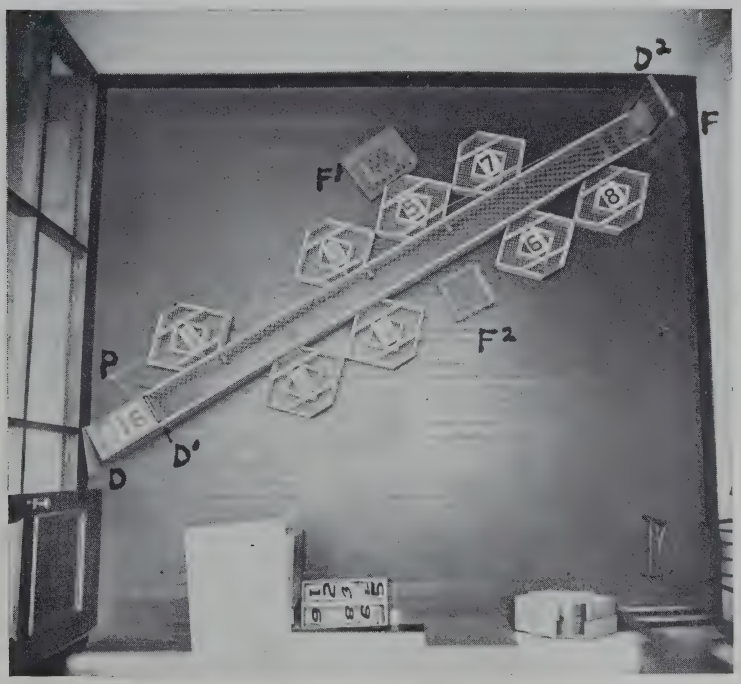

FIGURE 1

THE MIDWAY MAZE FOR ANIMALS

When food is at F^1 the animal can go directly to it or he can explore the "circle blinds" as he chooses

the end of the midway, and how promptly and accurately will he learn to go into and through 5 to the food cage. For the purpose of controlling odor diffusion in the room, food is placed also at F^2.

Along the inside edges of the midway path are placed 8 small blocks, 2 inches wide and 4 inches high, one at the entrance to each blind. These are for the purpose of interrupting the swift run of the animal along the wall, and to deflect him slightly into the blinds. At blinds 3 and 4, and 6 and 7, the blocks are opposite so that the path between them is 4 inches wide. Half-inch mesh wire screen covers the midway, as it does the various blinds. Both blinds and midway are without floor. In conducting tests with this outfit, the experimenter occupies a fixed position by the starting box.

The midway maze is offered as a means for investigating both the exploratory tendencies of animals and their learning ability. In this instrument the animal can go directly to the food, or he can enter several short by-paths, each one of which naturally leads him back into the main highway. At no place is he actually stuck, in the sense of having to back out. If he has excessive exploratory tendency, there is good opportunity to exhibit it. Perhaps this type of device is fully as typical of the normal environmental situation of an animal as is the usual maze.

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THE STEPPING-STONE MAZE: A DIRECTLY VISIBLE SPACE-PROBLEM APPARATUS

ROGER G. BARKER

Any learning problem involving spatial elements rather than such products of cultural experience as language offers a wide range of applicability in comparative, genetic, and ethnological studies. The maze situation has been so used extensively and seems to merit further use. As adapted to human subjects, however, the usual stylus maze presents serious drawbacks. Professor Miles in some recent papers has pointed out certain of these difficulties which tend to make the maze experiment annoying to subjects and unreliable in results. To meet the difficulty, largely a chance affair, of pressing the stylus on the right or the wrong side of the path and to substitute useful tactual experience for that of extended touch he devised the high-relief maze (6),[1] which has been used with so much success by Husband (2) and by Knotts and Miles (3) in their comparison of the maze-learning ability of blind and sighted children. Nyswander (10) has made a study of the high-relief versus the stylus maze of the same pattern.

[1]The idea of the high-relief or skeleton maze which is that of cutting out all needless cross-sectional area has been applied by Professor Miles (4, 7) to animal mazes.

A second maze-experiment difficulty has been solved by Professor Miles with his "unidirectional mazes" (8)[2], in which, after each choice, the hand of the subject is brought back to a median line. This maze has the great advantage that a diagram of it without the stops can be directly shown the subject at the start of the experiment. This feature of routine simplifies and standardizes the instructions and promotes a uniform attitude on the part of the subjects.

The most serious drawback in the use of small mazes with humans arises from the usual necessity of depriving the subject of vision. Carr (1) has offered a maze, with hidden stops, that can be looked at directly while the subject is solving it. Perkins (11) has developed the method of having the subject see only a small portion of the pattern through a tube, and Miles (5) has arranged a two-story maze that gives the maximum of indirect visual guidance.

The extreme importance of vision in dealing with spatial elements has recently been emphasized; it is sufficient to point out here that such a learning situation, which deprives the subject of the sensory data most efficient for dealing with these elements, is much more than a simple learning problem. Before learning can begin the situation has to be understood in terms of unpracticed, make-shift tactual and motor processes. And after the subject is oriented and learning is well under way these same relatively inefficient cues must guide motor movements in lieu of the customary visual guides. How much of the total score is attributable to the substituting of secondary cues for primary ones, and how much individuals and groups may vary in this substituting ability is not known; but anyone who has learned a maze, or who has observed other subjects will probably agree that this variability is large. Numerous comments such as, "If I could only see which way my finger is going;" or, "I know where I want to go, but how to make my finger go there is beyond me," substantiate this conclusion. Before the maze becomes valid for comparative studies of learning ability, the influence of these factors must be determined.

In devising what we have called "the stepping-stone maze" it has been our object to provide an apparatus which would make possible the presentation of maze learning problems freed from some of the former experimental difficulties.

Essentially the apparatus is 169 points arranged 13 x 13, one inch apart, and in one plane.[3] To the eye, the surface is homogeneous; the points are identical in size, color, and form, and their spacing is regular. One of the points is designated as the start, and the subject is told that a consecutive

[2]This idea he has also applied to animal mazes (9).

[3]The possibility of arranging curved surfaces or of making a solid of any desired shape and of covering it with such points and thus of providing a three-dimension maze is obvious.

line of points exends from that start. These points may be considered as markers along an imaginary path; if connected in order, the links between the points would form a single, continuous path. The subject's task is to discover this series of points and to remember their order and direction so that he can select them correctly; in other words, he is asked to learn to follow the imaginary path.

The construction of the apparatus is indicated by the illustrations, Figures 1 and 2. A 15-inch square piece of 3/8", three-ply panel forms the base; upon this is placed a covering of 1/16" battleship linoleum; this supplies the homogeneous surface. Upon the back of the panel a 13" square is laid out, and coordinate lines one inch apart are drawn. At the 169 intersections of these coordinates 1/8" holes are drilled, and in them are set 3/4", No. 6 round headed, blued, wood screws. The screw points project above the surface of the linoleum 1/32" and serve as the significant items on the homogeneous surface. The heads project below the panel 3/32" allowing ample space for wiring the true path and the error domains. The whole is framed with molding which projects below the panel 5/8" thus serving as a base supporting the panel.

The order and direction of the points from the start is determined, and these true points are wired on the bottom side by giving a wire a turn about

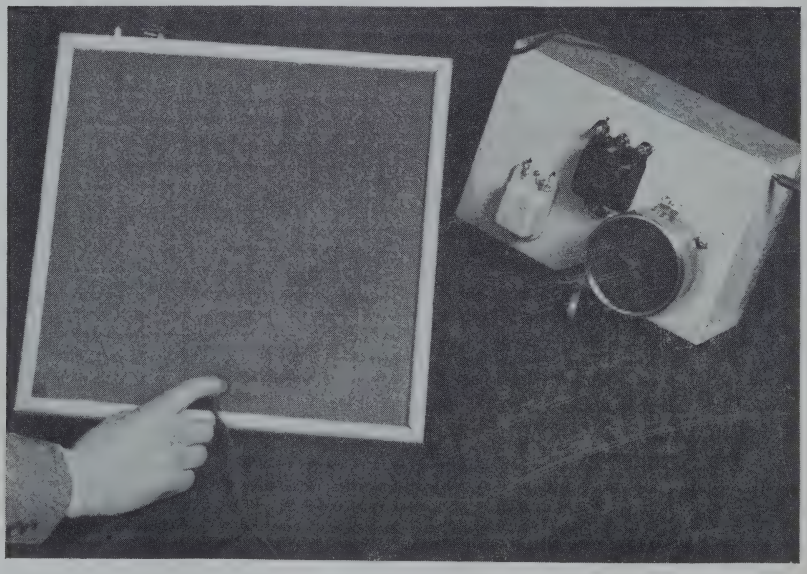

FIGURE 1
THE STEPPING-STONE MAZE WITH COUNTER-BUZZER UNIT

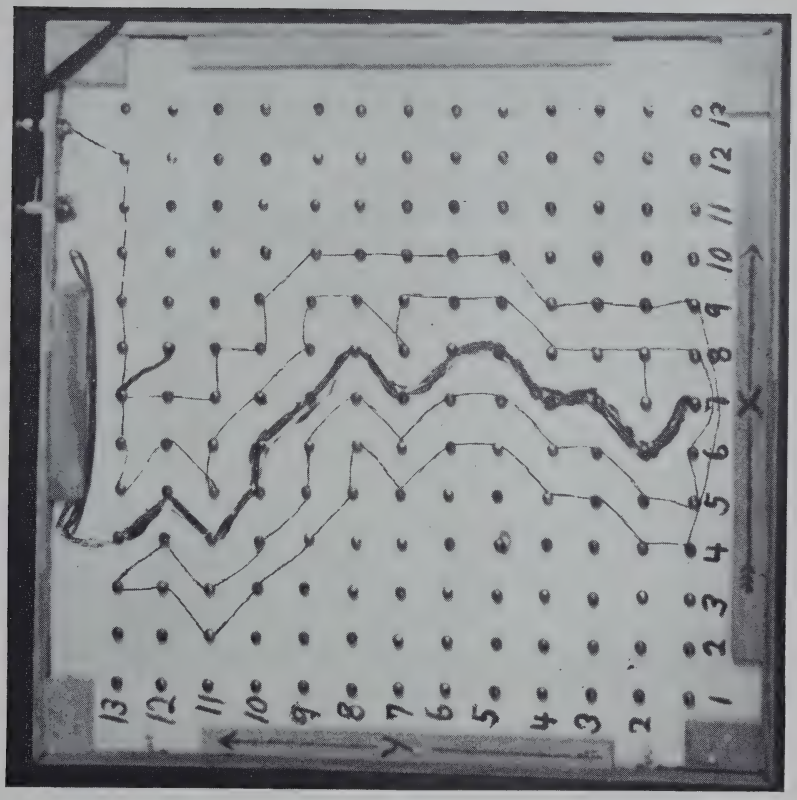

FIGURE 2

THE BACK SIDE OF THE STEPPING-STONE MAZE
On this side the patterns are set up by winding wires around the screw heads

each correct screw consecutively; this line of connected true points is then placed in circuit with a buzzer and a thimble stylus. When the subject touches a correct point with the thimble, the buzzer sounds.

The points contiguous to the correct points are wired in series also and placed in circuit with a counter and the same thimble stylus. These are the errors; when the subject touches a wrong point the click of the counter warns him of his error, and records it. A battery or a bell-ringing transformer supplies the current.

Some characteristics of this set-up as a learning problem involving spatial elements are:

1. Flexibility of pattern: The correct path may be easily changed;[4] a complete and complicated pattern may be wired in less than five minutes.

2. Ease of description and duplication of pattern: When the panel is turned over laterally, the bottom molding becomes the x-axis, the left molding the y-axis; the columns of screws are numbered from left to right; they are the ordinates; the rows of screws are numbered from bottom to top; they are the abscissa. The true points may be easily described in terms of these rectangular coordinates; e.g., the path wired as shown in the reversed panel Figure 2 becomes: 7, 1; 6, 2; 7, 3; 7, 4; 8, 5; 8, 6; 7, 7; 8, 8; 7, 9; 6, 10; 5, 10; 4, 11; 5, 12; and 4, 13.

3. Simple, automatic scoring of errors.

4. Compact, light; self-contained.

5. Motivation: The traditional maze has become a symbol for hopeless perplexity, and the laboratory maze is no exception. Comments by the great majority of subjects indicate this: "What's the point to this anyway? You don't mean to say it's possible to learn this! This means nothing to me. This is hopeless. There's nothing to tie to." Undoubtedly the usual maze presents a problem which at first seems blind and pointless. Sensory data have to be depended upon for guidance which in previous motor performances served simply as ground for dominant visual guides.

Hence the impression of "nothing to tie to;" the usual outstanding visual aspects are absent; new sensory aspects have to be found in the mass of less sensitive, less practiced, less attended-to motor and tactual data. The situation is not obvious; it has to be discovered in this unusual material before it has point or meaning. Under such circumstances, unless the subject approaches the problem with much outside motivation, interest and effort are likely to be feeble.

The spatial learning situation described above appears to remedy this difficulty to a considerable extent. The subject can "see" what he is doing; the old visual data habitually relied upon to successfully move in a spatial situation are present. There is no tedious straining to recognize tactual and motor cues previously not attended to; *the business of learning the points that buzz can be immediately entered upon.* The task seems easy; errors appear to be so unnecessary, yet they occur surprisingly often and are visibly recorded against one; there is a challenge. Comments of persons learning this problem are different from those learning the usual blind maze: "This is simple. Surely that wasn't wrong. Look here, you must be changing the path on me. Now just wait; I'll have it the next time."

6. The apparatus may be used for other than simple learning problems.

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[4]Alternate patterns can be wired up at the same time and ready at the change of a switch.

The path may be wired in accordance with any one of innumerable principles; e.g., alternate directions regularly; or, first choice, one point to left; second, two to right; third, three points to left, etc. There are many such systems available even on a field of the size illustrated.

A variation of the "Ball and Field" problem in the Stanford-Binet Test may be obtained by wiring several points in the field in the buzzer circuit; the person tested is asked to find them.

7. The subject always knows exactly what movement possibilities are before him, the only problem being to remember which of the obvious possibilities is the correct one. This is opposed to the maze in which at any point it is necessary by successive motor explorations to determine the nature of the situation.

8. Visual guidance versus motor guidance: The subject always knows which choice he is making as is often not the case with the blind maze—vision is the competent supervisor of action.

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INEXPENSIVE APPARATUS FOR CONTINUOUS CHOICE REACTION

HAROLD E. BURTT

There are numerous uses for an apparatus which will provide a continuous choice reaction. By this is meant an experimental situation in which the subject performs a choice reaction with two or more telegraph keys to two or more stimuli in such a manner that every correct response presents the next stimulus automatically. Various devices have been used for this purpose—some of them mechanical and some electrical (cf. 1). In the present instance an electrical arrangement was desired with two visual stimuli and two keys. After some annoying experience in constructing and particularly in attempting to adjust a punched cardboard disc apparatus which was operated by a magnetically controlled ratchet stepping device with four rollers making contact through the holes, the following arrangement was hit upon. The total cost was under $5.00, the total labor was less than two hours, and the apparatus worked the first time.

The principal unit is a rotary line switch, such as is used in automatic telephone service. Units of this sort are available from companies that handle second-hand telephone equipment at a price of about $3.00. This switch involves four banks of 25 contacts each, arranged in a semicircular mounting. These contacts are all insulated from one another and also from the frame. The contacts are on the inside of the semicircle, but each extends through the outside of the mounting and terminates in a soldering lug. The rotor at the center of the semicircle is operated by an electromagnetic ratchet stepping device. It has four pairs of double-end contacting fingers with each pair on opposite ends of the same diameter. These fingers are all in a plane through the axis of rotation so that at any given moment the four fingers are touching four of the stationary contacts—one contact in each bank. When the last set of fingers on one end of the diameters runs off the semicircle, the fingers on the other end of the diameters make the first contacts on the other end of the semicircle. Each impulse through the electromagnet sets the rotor along one notch, moving it from one set of contacts to the next.

The only alteration of this rotary switch necessary for the present purpose was to solder together certain lugs on each bank and to ground the four members of the rotor. This has the effect of leaving certain contacts on each bank dead and is simpler than removing them. The wire soldered to contacts in each bank is brought out to a binding post—four in all.

The rotor is connected to one side of the 110-volt d.c. source. Two of the banks are connected to the two stimulus lamps. The other terminals of the lamps are connected together and to the other side of the source. When a finger on the rotor engages with a contact that is connected to the

rest of the bank the corresponding stimulus lamp lights. Obviously the original connecting is done so that only one light will appear at a time.

The other two banks are connected to the telegraph keys. The other terminals of the keys are connected together and lead through the magnet of the stepping device and a lamp resistance (60 watts) back to the common line coming from the stimulus lamps to the source.

The particular contacts which are wired together in a given bank determine the order in which the stimuli will be presented. One of the stimulus banks and one of the key banks are identical with regard to the contacts that are connected together. For example, if contacts 1, 3, 6, 7 and 9 are connected in one bank going to the left light, the same contacts 1, 3, 6, 7 and 9 in the bank going to the left key would be connected. Contacts 2, 4, 5, 8, etc., on the other two banks would lead respectively to the right lamp and the right key.

Suppose, for example, that the left lamp is lighted and the subject is to press the left key for a correct reaction. If he presses the wrong key that circuit through the key is dead because the contact on the bank leading to that key is not connected to the rest of the bank but insulated. Consequently nothing happens. If, however, the subject presses the correct key the contact on that bank, as explained above, is alive and connected to the rest of the bank. The result is that current goes through the key and the magnet of the line switch and the rotor steps along one contact. This immediately connects the next light and also the circuit to the corresponding key for the next reaction. The incorrect reaction does nothing but delay the subject and every correct one moves the rotor along one notch. He is thus scored on the number of correct reactions made in a given period such as one minute. If the correct stimulus is on the same side several times in succession there is a slight flicker in the light as the rotor shifts and he can also hear the click of the apparatus if it is not enclosed in a sound-proof box.

There is a small graduated dial on the rotor of the line switch with a marker at the margin so that it is possible to read the number of contacts made directly from this little scale. It is necessary, however, to watch rather closely to see how many times the thing has rotated, because the scale only reads up to 50. It is more convenient to insert a separate electric counter in series with the magnet of the stepping device. If the constants of the magnets are not correct it may be necessary to shunt one of them with a small rheostat.

The foregoing are the essential features of the apparatus. The nature of stimuli and keys are at the discretion of the experimenter. We are using circles of light 1/8 inch in diameter—holes in brass plates on the front of boxes in which the lamps are mounted—and two keys of spring brass with finger buttons operated by the index and middle fingers. The keys rock sc

that they cannot be operated simultaneously. The average subject under these conditions makes around 100 correct reactions a minute.

The advantages of the apparatus, aside from its small cost and labor, are that the stepping device works perfectly at any speed conceivable for this type of reaction and that the fingers wiping on each side of the contact make a very certain connection.

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H. E. GARRETT. *Great Experiments in Psychology.* New York: Century, 1930. Pp. xvii+337.

The task of providing a judiciously selected set of collateral readings for an introductory course in general psychology is not a simple one. If the material is to be experimental in nature, and presumably that is desirable, the difficulties are multiplied. Professor Garrett intends the present volume to meet such a need. The specific demands of an instructor in teaching general psychology stand as the only criteria of the usefulness of the book and one's reaction to it is necessarily an individual one.

Without bringing into question the meaning of "great" in the title (the author asks our indulgence here, since what is to be called "great" is also a matter of individual opinion), the title of the book may be challenged in terms of its content. A better one would have been "Great Experiments in Educational Psychology." At least, fewer changes in the body of the text would have been necessary to make the material fit such a title than would seem to be required to allow the present one to conform to its content. While much of the material of each chapter is so presented as to indicate wider implications, more than half of the fourteen chapters comprising the volume are primarily concerned with experiments, the generalizations from which are of the greatest service to educational rather than general psychology. Many of the remaining chapters are strongly educational in their bearing. It is, of course, a violation of one of the canons of criticism to object to what an author has set out to do, but criticism is justifiable when he swerves from his aim.

The material itself, once it is recognized for what it is, rather than what it purports to be, needs little defense, nor does the manner in which it is presented. From the consideration in the first chapter of Binet's work to the exposition of the experiments of Lashley and Franz in the last the

stories of famous experiments, together with their settings are simply and straightforwardly told. There is a successful avoidance of bothersome technical terms without a reduction of scientific significance. Throughout the author displays the too uncommon flair for stating psychological principles and procedures in terms which are understandable to the beginning student. A nice balance has been realized in that this characteristic is made to harmonize with a fund of information that should prove provocative of thought.

The work of Binet and the war "experiment" with Army Alpha occupy the first two chapters. The third is devoted to the famous memory experiments of Ebbinghaus. Like so many other accounts in the book, this chapter is enhanced by a statement of the more recent work growing out of the problems set by Ebbinghaus' monograph. A rather thoroughgoing account of the work of Pavlov and his students occupies the next. The story is amplified by the indication of the place of the conditioned reflex principle in modern psychological theory. The next four chapters on "Thorndike's Animal Experiments and the Laws of Learning," "Thorndike and Woodworth's Experiment on the Transfer of Training and its Influence Upon the Doctrine of Formal Discipline," "Watson's Experimental Studies of the Behavior of the Human Infant," and "Galton and the Measurement of Individual Differences" are not notable. There is displayed a tendency on the part of the author to become lost in review of the recent literature; by implication, some of the "great" experiments are damned by too faint praise. A faithful description of the reaction-time studies of Cattell and his students is given in Chapter Nine. The author has not availed himself of the opportunity to add considerable interest to the story by recounting in a detailed manner the origin of the "personal equation" problem in astronomical measurement. The chapter in Boring's history is much more readable for its inclusion. One feels that a measure of the greatness of the reaction experiments is the number of technical problems and special relations to which this work drew attention.

The work of Cannon certainly belongs in any list of great experiments. Garrett has minimized the amount of space devoted to these researches and has thus seemed to reduce their importance. Here again his purpose seems to be that of digesting the literature. The treatment of Gestalt psychology in the next chapter merits commendation on the ground that it not only describes the more important experimental contributions of those responsible for this movement but presents in a brief manner as clear an account of *Gestalttheorie* as the reviewer has seen anywhere. It is not easy to present this simply yet meaningfully; the evaluation of the movement as given by Professor Garrett is sane. A surprisingly large amount of information is packed into the chapter called "Weber's and Fechner's Laws and the Rise of Psychophysics."

It is perhaps impossible in a book which is intended to find its usefulness as a companion volume to an elementary text to avoid in places the kind of material that belongs in the text itself. The inclusion of too great an amount of such material is not a virtue, however. The chapter on "Experimental Studies in the Visual Perception of Depth" errs in this direction. There are surely many more great experiments in this field than the author has seen fit to include. The description of them could have replaced with profit the exposition of elementary dioptrics, for example. Only five of eighteen pages in this chapter are devoted to accounts of experiments. The last chapter, dealing with the various studies that have brought us to the current conception of vicarious cortical functioning, is admirably done. The experiments of Franz and Lashley are described in a simple and succinct manner.

As the author observes in his preface, "the percentage of undergraduates who go to the literature . . . is lamentably small," and the present work is well suited to catch interest where otherwise it might be wanting. In cases where the subject matter of a beginning course leans heavily towards the educational, and where predigested literature is a desideratum, the book is likely to prove decidedly useful.

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